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ORIGINAL LECTURES.

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LECTURE I.

ON VITAL AND MEDICAL STATISTICS.[1]

I PROPOSE in these lectures to speak of vital and medical statistics, and of some of their relations to each other and to scientific and practical medicine and sanitation. The discussion will include such points as, character of the data required, methods of obtaining them by the census, by registration, and in other ways; relations of physicians to this kind of work, methods of compilation and forms of publication, the best existing sources of such data, and some of the more common fallacies in drawing conclusions from the data as ordinarily published.

These and other points will be considered in their practical applications to certain questions which I hope may be of interest to you, as, for example: Is the average longevity of man in civilized countries increasing? What data are required to judge practically of the relative healthfulness of different localities, or of the same locality at different times? What are the relations of certain forms of disease to race, to climate, to locality, to occupation? What is the relative tendency to increase of population in this country in the white and colored races? What is the statistical evidence with regard to improvement in practical therapeutics as derived from hospital data, from death-rates in obstetric practice, etc.?

Statistics, and discussion of statistical methods, are, as a rule, dry and uninteresting subjects, and it is with very considerable doubt and hesitation that such a topic has been selected for these lectures. I have no new discoveries to announce, and those who are practically familiar with statistical research will find some of my statements rather elementary; but the subject is not one which lies within the ordinary range of medical studies; the data are widely scattered in literature, and I hope at least to be able to remind you of some of the numerous points which you may have once known, but which may have been forgotten owing to the pressure of other studies and duties.

Circumstances have made it necessary for me to endeavor to ascertain the character and amount of the statistical data which have been published, and which are available for those who wish to make special studies of the relative frequency of certain diseases, deformities, or disabilities; of their causes and results, especially as to mortality; or of the relations which exist between disease and death, and such conditions as sex, age, race, soil, climate, occupation, etc., and in the course of this investigation it has seemed to me that many physicians are not as familiar with statistical methods and with existing sources of information relating to vital statistics as it is desirable that they should be to enable them to judge properly the value of the arguments and conclusions presented by those who make use of such data, even though they themselves may have no desire or intention to use statistical methods of research.

There are many fallacies and errors connected with vital and medical statistics as ordinarily collected and used, and it is highly desirable that the physician should be aware of the more important of these, since he is constantly appealed to for decisions as to their true significance and value. "It is as easy to tell lies with figures as with words, and bigger ones;" but while we occasionally meet with deliberate falsifications of the records, made for the purpose of magnifying or diminishing the apparent mortality or prevalence of a particular disease in a given locality, or to maintain an anti-vaccination thesis, these are not so frequent as are the errors of involuntary misstatement and misinterpretation into which those not familiar with methods of collecting and tabulating statistics are so liable to fall.

My experience with those seeking statistical data is, that the majority begin by looking for data which are in favor of some particular conclusion with which they commence, rather than by selecting data with reference to their probable completeness and accuracy, and accepting the conclusions which may be legitimately drawn from them, whatever they may be.

Those who are engaged in the collection and compilation of official mortality and vital statistics are often at first the most sceptical as to their accuracy and utility, for their attention is so frequently and forcibly drawn to errors in the individual data that they conclude that the whole mass is unreliable, and the difficulties in the way of obtaining complete and reliable figures are seen to be so great that they incline to give up the whole matter in despair. Continued study of the subject, however, shows that many valuable conclusions or suggestions can be derived from imperfect data, and that in large masses of figures the errors either tend to neutralize each other, or to produce a constant effect in one direction which can be calculated and allowed for, so that those who have had the greatest experience are most convinced of their value. It is true that, in statistics, the inferences cannot be more accurate than the data on which they are founded, but we do not look for scientific exactness from them so much as for an estimate of probabilities.

[1] Properly speaking, there is no such thing as the science of statistics in the sense in which that term is used by French and German writers, but there is a method of statistical analysis which may be applied in a number of fields of science, and which is essential in the study of sociology.

The methods which we have for advancing our knowledge of the laws of human life, of the causes of abnormity, disease and death in man, and of means of sanitation or therapeutics, may be grouped into two classes, viz.: observation and experiment, which, however, are often combined.

In the experimental method we seek to make a direct test of the variation of one particular condition, or set of conditions, upon the living organism, all other conditions being kept uniform as far as possible. Some such experiments can be made on man, but the greatest number of the problems which we may hope to solve by this method, and among these the most important, can only be approached by experiments on the lower animals. Within the last twenty years, experimental physiology and pathology have made great advances, and these methods, so far as they are applicable, give more definite results, and are more immediately satisfactory, than those derived from comparison of observations in which no definite experimental variations have been made; but so far as regards causes of disease, or the action of supposed methods of prevention, or of remedies, it is unfortunately the case that we cannot draw accurate conclusions as to what will happen in man from what is observed to happen in animals. In the first place, there are many important forms of disease in man which cannot, with our present knowledge, be experimentally produced in animals, and which rarely or never occur in them.

For example: Yellow fever is a disease which, from analogy, we have every reason to believe is due to the action of one or more specific microörganisms, or, perhaps, I should say, to the products of such organisms. We find a dozen different kinds of bacteria in persons suffering from yellow fever, and, by dint of much labor, these have been isolated and cultivated outside the human body. The problem is to determine positively, and with scientific precision, which, if any, of these is the true essential cause of the disease. The mode of doing this is by producing the disease in a perfectly healthy person or animal by the inoculation of the suspected organism. But, thus far, we have failed to find any animal in which a disease, which can be considered as specifically identical with yellow fever, can be produced by any method; and I need hardly tell you that inoculations of such a disease as this in a human subject, under conditions which would make the results of such inoculation scientifically trustworthy, are impracticable and unjustifiable.

Those forms of disease which are common to animals and man, such, for instance, as anthrax, tuberculosis, tetanus, hydrophobia, the ordinary forms of suppuration, and, possibly, also typhoid fever, are being pretty thoroughly worked out by means of such experimental inoculations as I have just referred to; and we are able to say, with a great degree of precision, not only that these diseases are due to specific forms of bacteria, but to determine enough of the characteristics of these forms to be able to identify them wherever they are found.

For the great majority of diseases, however, this experimental method of determining the causes, and, therefore, necessarily, experimental methods of prevention and treatment, cannot be employed in such a way as to isolate distinctly the effect of the particular agent or circumstance which we are investigating, and we must therefore resort to the other mode of advance of knowledge which has been referred to, namely, that of observing the phenomena as they occur in man, and endeavoring so to group the results of these observations as to determine the influence of a particular condition or set of conditions while all other circumstances remain uniform.

This method of observation may, for our purposes, be again divided into two categories. The first is that which is used in individual cases, being the form applied by the physician to each case which he has to treat. It also includes the sort of investigation which may be made in a single household, a small community, or a thinly populated district to determine the course and cause of a particular form of endemic or epidemic disease, where the conditions affecting each family or dwelling can be studied in detail. By the combination and comparison of detailed studies of this kind, the greater part of our present system of diagnosis, prognosis, and therapeutics has been evolved; but it has been and will be a slow process, for each man differs from every other man in structure and mode of function, and the conditions of the environment are so multiform, and so variable in space and time, that "experience is doubtful, and judgment difficult." We must, therefore, try to supplement the information thus obtained by that derived from the second kind of observation above referred to, namely, that of collecting a few data with regard to great numbers of people, especially where these are accumulated in thickly settled localities, forming what is called the statistical method as applied to different communities. By the first method we compare individual with individual, and do so with considerable minuteness of subdivision of the conditions studied; by the second method we compare the vital phenomena of communities with those of other communities, but only on broad lines and in relation to circumstances easily noted.

The object of vital statistics is to classify and arrange the facts relating to the quantity and character of human life under different circumstances, for the purpose of determining the effect upon it of each of these circumstances taken singly, or of two or more of them acting together. The results thus obtained form an important part of the scientific foundations of sociology, of political economy, and of preventive medicine. It deals with masses of men and not with individuals, and its conclusions are, for the most part, applicable only to large bodies of people; yet its data are derived from individual records, and its results are accepted in many cases as a sufficient guide for individual action. They do not, in this respect, differ essentially from most of the motives which actuate us in daily life. The farmer sows his seed, the manufacturer builds his mills, the physician writes his prescriptions, and you come to hear this lecture from motives of probability, not carefully formulated in most cases or capable of expression by a formula, but, nevertheless, in accordance with the doctrine of chances as determined by previous experience.

The term "vital statistics," in the sense in which I use it in these lectures, corresponds almost precisely to the French term "demographie" or the German "demologie," being applied to the circumstance of human life only in the sense of Korosi's definition that it is the science of the physical life of human society. We will first consider briefly the sources of information and the character of the original data available for this study;

second, the methods of arranging and using these data; and, third, the value and applicability of some of the conclusions which may be drawn from them, especially in that branch known as medical statistics.

The essential data of vital statistics are derived from enumerations of the living population and from records of births, marriages, and deaths.

The numbering of the people is effected by a census, a term derived from the Roman Censors, a part of whose duty was to make such counts. Such enumerations were made by Moses (1490 B.C.), David (1017 B.C.), in Greece, 650 B.C., and in Rome beginning 566 B.C. In modern times the first country to make a count was Sweden in 1749. The first census in the United States was taken in 1790 as a necessary means of carrying out the constitutional provision that the basis of representation for the several States should be the number of the population in each. The first census in England was taken in 1801, and showed the number of persons, with distinction of sex, the number of houses, the number of families, and a rough statement of occupations under the general classification of agriculture, trade, manufactures or handicraft, and all others.

The first satisfactory English census, giving distinction of age, sex, occupation and birthplace, the number of persons blind and deaf and dumb, was the census of 1851. This was the first census whose data could be used in connection with the general system of registration of births and deaths.

The first attempts to take a census in a country have usually excited more or less suspicion and opposition, from fear that the information obtained would be in some way used to oppress the people. For example, several attempts to take a census have been made in Hayti, but have always given rise to insurrection, and have always been defeated, owing to the belief of the people that it would in some way lead to their losing their liberty.

In the first censuses the object was to determine the number liable for military service or qualified to vote, or to fix rates of taxation, and the records were very brief. In the first United States census the only data called for on the schedules were, names of heads of families, free white males over sixteen, free white males under sixteen, free white females, all other free persons, and slaves.

The record of age was first made in the English census in 1821, was omitted in 1831, and resumed in 1841. As the census records came to be more and more relied upon as a basis for legislation, additional details were introduced, until at the present time we have such records from some of the principal countries of the world.

The relative importance of each of these details in studies of vital statistics varies greatly according to the purpose to which they are to be applied; but to physicians, sanitarians, and those interested in life insurance, the do inating factors are age, sex, and race. The power of reproduction, the tendency to death, and the liability to certain forms of disease, vary greatly at different ages of the two sexes, and this gives rise to corresponding variations in the disease, and death-rates of populations of different localities, or of the same locality at different times, when those populations differ as to the relative proportion of young and old or of male and female which they contain. These variations we must ascertain if possible, and estimate the influence of, in order to make reliable and useful studies of the effects upon human life of climate, altitude, race, occupation, or other conditions of the environment. In the course of these lectures I shall often have occasion to refer to this dominating influence of age, and to point out errors due to neglect of this factor, which is by no means an easy one to determine for many places at many times. Even the census data require corrections, since people do not give their ages accurately to the enumerators. The tendency is to answer in round numbers, as 20, 30, 40, etc., and to a less but still marked extent, as 25, 35, 45, etc.; but the effect of this can be done away with to a considerable extent by properly grouping the individual data, as I shall explain presently. Ages over 90 are largely overstated, and the ages of women between 25 and 50 are largely understated; but those errors being comparatively constant, and influencing the data of all communities, do not lead to erroneous conclusions of importance in comparing different communities, if the distinction of sex be observed.

One of the most interesting fields of study in vital statistics is the relation of race and color to birth-rate, to certain forms of disease, or to the liability of death at certain ages.

This country is, as you know, the great mixing ground for different races of the human family, and, while the mixture is rapidly becoming so intricate as to make it impossible to distinguish the several strains, it is still true that there are large groups of men of quite distinct races, the record of disease and death in which would form valuable material for study upon this point were it possible to collect them.

From the sociological and political point of view this is particularly the case with regard to the negro and to those having a mixture of negro blood; and in the Southern States such questions as the following are of great practical interest: Is the negro population increasing faster than the white? Is the proportion of mixed bloods, such as mulattoes, quadroons, etc., increasing in proportion to the general population? Are the fertility and expectation of life of mixed bloods greater or less than those of pure whites or pure blacks under the same circumstances of environment? We will return to these questions hereafter; at present I merely refer to them as being the probable reason for the introduction into the law for taking the next census of a special clause providing "that the population schedule shall include an inquiry as to the number of negroes, mulattoes, quadroons, and octoroons," an attempt to obtain information which has not heretofore been sought in this way. In obtaining the records of deaths occurring during the census year beginning June 1, 1889, an effort will be made to have the deaths of colored persons distinguished into those of pure negroes and those of mixed blood. It will probably be impossible to obtain the data for either living population or deaths with the minuteness of subdivision indicated by the words "mulatto," "quadroon," and "octoroon"; but there is reason to hope that in many sections of the country we shall be able to distinguish those of mixed blood from the pure blacks and the pure whites, and to give some opinion with regard to their diseases and death-rates.

The results of the last census, although imperfect, show such marked differences as regards the mortality from certain diseases, not only between the whites and the blacks, but between those of English, Irish, and German descent, as to make it certain that it will be worth while to pursue this branch of inquiry more minutely as opportunity is offered to us hereafter.

The influence of race upon mortality is specially manifest in the death-rates of cancer. The number of deaths from cancer per 100,000 population in certain portions of the United States, with distinction of white and colored, was as follows:

White.	Colored.
27.96	12.67

In the northern part of the United States the proportion of deaths from cancer in proportion to 1000 deaths from known causes, with distinction of white, colored, Irish and German parentage, was as follows:

White.	Colored.	Irish parentage.	German parentage.
19.1	7.8	24.3	25.8

It will be seen from these that the liability to death from cancer is not half as great among the colored people as it is among the whites, and that there is a greater tendency to death from cancer in persons of German parentage than in all the average white population, especially between the ages of fifteen and sixty-five.

The relation of race to vital phenomena in general, and to disease and death-rates in particular, forms one of the most interesting branches of what Galton calls the "science of heredity," but it is a branch in which little progress has been made, and for the study of which the United States offers greater opportunities than any other country. "The question of race influence is not merely an abstract matter fitted only for well-rounded periods in the discussions of the schools, but it profoundly affects vital and national life." It is a force which acts incessantly upon and menaces us, and, so far as we can now see, it is mainly upon the outcome of the distribution and prevalence of race that depend civilization, religion, and the future of man upon the earth. In so far as the conditions of things tend to preserve the best types, progress is favored. In so far as they tend to destroy or to debase them with inferior types, progress is hindered. Not every mixture of race prevails, or persists, but there has been a certain amount of mixture wherever there has been progress in human affairs. Such mixture appears to have been a consequence rather than a cause, yet it may become an important secondary cause in changing or modifying the course of human events.

The census gives us a view of the population on a certain day, and, if well taken and properly applied it gives a general view of the stream of life as it flows on that day, with its variations of breadth and depth, from which it is possible to calculate, within certain limits, the velocity of the current, the rapidity of change, and the probable rate of increase or decrease, especially if comparisons can be made with the results of a previous census taken in the same way. It may also indicate periods of widespread disaster or of migration.

The proportions of the living population in 1880 show that the decrease in the number living at each quinquennial group of ages was tolerably regular for the whole population for the native-born whites and for the colored as we proceed from the lower to the higher ages, as it should be under ordinary circumstances; but that there is one marked exception for the age-group 15 to 20, in which the line makes a sudden angle, indicating a relative deficiency in the number of persons living at this age.

It will be observed also that the age-group in which there is the largest proportion of the foreign-born population is that from 35 to 40.

Comparing these figures with those of the census of 1870, we find that this peculiar deficiency in the age-group, and the maximum proportion for foreign-born occur in those age-groups which precede by ten years the groups in which they occurred in 1880. The break or step in the descending line is in the age-group 5 to 10, instead of that 15 to 20, and the maximum proportion of the foreign-born is in the age-group 25 to 30, instead of that 35 to 40. Now if we go back between five and ten years from the census of 1870, to see what special cause existed in that period for a diminution in the number of births, we find ourselves in the period of our civil war. These breaks or angles, then, are the scars of one of the wounds which the war inflicted. The shifting of the maximum point in the line of the foreign-born population indicates the unusually great immigration of Irish and German families containing young children, which occurred between 1850 and 1860, forming a wave whose crest is still perceptible. It is safe to predict that in the census of 1890 the break or step showing a deficiency in births will be shifted forward to the age-group 25 to 30, and that the maximum proportion of the foreign-born will be in the group 45 to 50.

In general, however, we may say that the census indicates the state of the population at a given period. Vital statistics, however, consider both the state and the movement of the population, and, therefore, for these we must have something more than the census, viz.: a record of the deaths and births occurring in successive periods, from which we can compute mortality and natal rates.

Mortality, or mortality rate, refers to a ratio between the number of deaths occurring and the number of living population furnishing those deaths. It is to be distinguished from a statement of the number of deaths, since to determine the mortality in a given population, we must not only know the number of deaths, but also the population furnishing that number. This may be expressed in the following formula: $M = \frac{D}{P.}$

In the same way natality does not mean the number of births, but it means the ratio of the number of births to the population in which they occur: $N = \frac{B}{P.}$

The relations between mortality and natality are very important, as I shall have occasion to explain hereafter. The value of such statistics depends, of course, on the accuracy of the individual data and the completeness with which these data are gathered for the given locality to which they relate.

Accurate data with regard to deaths can only be obtained by a system of registration of deaths made at the time they occur. Repeated experience has shown that it is utterly impossible to collect, at the end of a year, by any mechanism of enumeration, more than 70 per cent. of the deaths which have occurred during the preceding year; and it is now well recognized that a complete regis-

tration of deaths can only be secured by legislation which forbids a burial until a permit has been granted from a central office, which permit is issued only on the certificate of a physician, setting forth the cause of death and other facts connected with it which are of importance, and which will presently be referred to. In the great majority of cases it is comparatively easy to enforce the law, even in thinly settled rural districts, and the community soon learns to consider any attempt at burial without a permit as a suspicious circumstance, indicating a desire to conceal either the death or the cause of death, and justifying a special investigation by the authorities. When it has been decided to require a burial permit in all cases, it is not usually difficult to require the data for registration as an indispensable preliminary to the issuing of such permit.

Any system which depends upon the returns of undertakers for a record of deaths gives incomplete and unsatisfactory results. It is only where the permit must be obtained before burial, and the certificate must be filed at a central office before the permit is issued, that a complete record of all deaths will be obtained. Any complete system of death registration should include some method of verification of the death and of its cause, which must be certified to by some person having the special knowledge which alone can enable him to give such a certificate.

In the first place, we must have this verification to insure the fact of a death having taken place. In its absence, in a large city, there is little or no difficulty in having recorded the death of a person who may be either alive and well, or non-existent, and the door is thus opened to frauds of various kinds, some of which have actually been attempted and discovered, while others, no doubt, have been successful and remain still unknown. Such verification is also necessary to insure the fact of real as opposed to apparent death in any case, and thus prevent premature burial,

The utility for this latter purpose is, of course, small, for the popular idea as to the frequency of trance or other conditions simulating death, so that the true state of affairs is not detected, is, as you all know, grossly exaggerated. Nevertheless, this consideration may enter as a factor into an argument in favor of such skilled verification.

The main reason, however, for the verification of a death by expert testimony as to its cause is, that it is necessary to establish the fact that a death has taken place from what may be called natural causes as opposed to criminal causes.

This verification of death, and of the causes of death, may be made either by physicians employed for that particular purpose and paid by the State, or by the physician under whose charge the deceased person has been immediately before death, in which latter case only those cases which have not been under the treatment of a physician are referred to a public medical officer, or the coroner, for verification and determination of the cause of death.

The first system is that which is employed in France, Austria, and Belgium. The second is the one made use of in England and in this country.

All registration laws include the certificates of physicians as an essential part of their machinery. Some do this directly, requiring that physicians keep a list of all deaths occurring in their practice, and shall forward this list at stated times to the registrar. This method has invariably proved to be a failure, as has also the similar attempt to require of clergymen that they shall furnish lists of the marriages which they have solemnized. It is utterly impossible to enforce such laws under penalties, and not fifty per cent. of either clergymen or physicians will carry out their requirements under ordinary circumstances.

Where burial permits are required, a physician may be made responsible for a certificate as to those matters only with regard to which his special professional knowledge is necessary, such as the cause of death, duration of sickness, etc.; or he may be required also to certify as to the age, birthplace, parentage, occupation, etc. The great majority of physicians accept without hesitation the data furnished on these points by some member of the family, or whatever appears set down in the form of a certificate brought to them by the undertaker for signature. But there are always physicians who question the propriety of the law, and object to certifying to that of which they can have no personal knowledge, while some few may possibly decline.

The requirements of a registration law impose upon medical men who sign certificates as to causes of death a very considerable responsibility, much more considerable, in fact, than many of them probably realize. The physician is to consider whether his knowledge of the case is sufficient to enable him to determine whether or not the death was due to what are called natural causes, whether there is reason to suspect that violence, poisoning, criminal neglect, etc., may have been more or less factors in the result, and whether any certificate as to the nature of the cause is justifiable. The pressure upon the medical man to certify to more, or, sometimes less than he knows, is occasionally very strong, but the only course in doubtful cases is to indicate clearly what one knows, as distinguished from what he merely believes on the faith of statements made by others. In ordinary matters of daily routine occurrence, in which there is no apparent motive for falsification, we constantly do and must accept the statements of others, the physician acts as the primary judge of the evidence submitted by relations and friends as to the time of death, the age and race of the decedent, the duration of the disease, etc., and is justified in certifying to his belief in this evidence, very much as he is justified in certifying to the date of his own birth.

It has been objected[1] that registration laws may do very well for countries where people have been trained for generations in that line of action, but that they are at war with principles of democratic action and with individual freedom, and that the reason why physicians do not execute the law is because they not only have no personal interest in its execution, but because of a feeling of revolt against the injustice of a law which inflicts a special tax in the shape of time and trouble, and affords no compensation for the extra labor and expense.

These views are not those held by the majority of the medical profession, and it is not probable that the payment of a fee would add much, if anything, to the completeness or accuracy of the registration data furnished

[1] Dr. H. M. Lyman, in the Chicago Medical Journal, 1878, p. 252.

by medical men. Nevertheless, these objections have some weight from a legal standpoint, and should be borne in mind in attempts at legislation. Any attempt to compel a physician, under penalty, to report the age and birthplace of his patients, would, certainly, be worse than useless. The policy is not to call upon medical men to submit the information which should be demanded from the parent or householder. Under the police power of a State, certificates of the cause of death may be required from physicians as being necessary to secure life and protect property, but returns for merely statistical purposes, such as of births, cannot be required of any other than the parent.

There is no good reason why reports of births should be required from medical men. But as regards reports of deaths, it is to the interest of properly qualified members of the medical profession that such certificates should be demanded from them. Whenever and wherever certificates as to the cause of death from physicians are required, there must also be established some system of determining who are physicians within the intent of the law.

At first it may be necessary to accept certificates from anyone and everyone who chooses to call himself or herself a physician; but the character of some of the documents of this kind which will come in, will very soon indicate the necessity for some discrimination. Thus it is that the certification of the causes of death by physicians is the essential foundation, and it is the only essential foundation of legislation with regard to the qualifications which the State has a right to demand from practitioners of medicine.

The registration of marriages, births, and deaths is important to the individual, because it gives him increased security in his rights to property and to life, by enabling him to furnish proof of parentage and legitimacy, by increasing the chance of detection of fraudulent claimants to property of which he is the true heir, and by discouraging criminal attempts to shorten his life, owing to the fact that evidence must be furnished that death was due to natural causes, or a special legal investigation of the circumstances will be made. Of its importance to the community as a means of protection of health and life, and to scientific men and physicians as a means of investigation of some of their problems I need give no proof to this audience.

We can hardly be said to have a complete system of registration of births in any State or city in this country. Probably, the city of Providence, R. I., has the most complete records of this kind of any of our cities. As regards the registration of deaths, Massachusetts, New Jersey, the greater part of Connecticut and New York, a large part of Alabama and Minnesota, and most of our large cities, have now a fairly satisfactory system and complete record. For the rest of the United States, there is either no system of registration, or, if any exists, it is a very imperfect and incomplete one, the results of which cannot be depended upon, and which cannot be compared with the results obtained in the localities above referred to as having a complete system, and the only means which we have of estimating the mortality of these localities is by the reports of deaths for the preceding year collected by the census enumerators.

It is for this reason that the decennial United States Census is a matter of such great importance to scientific medicine and to practical sanitation—of much greater importance, in fact, than most physicians and health officials seem fully to appreciate. It is true that the death records thus obtained in the large areas of the country in which there is no registration are incomplete, and, as regards causes of death, especially inaccurate; but they are the best we have; they are becoming better at each census, and the death records in the registration areas serve to measure their reliability, and to indicate to some extent useful corrections.

As the value of statistics of deaths depends very largely upon the possibility of comparing them with corresponding statistics of the living population furnishing those deaths, it is evident that the modes and times of obtaining and of publishing the results of the census are matters of great importance to medical and sanitary statisticians. This is especially true as to the frequency with which a census is taken, the units of area made use of in its published tables, and the combinations of age, sex, race, and occupation data given in connection with such units of area.

Let us first consider briefly the time of taking the census.

The conclusions of the various statistical congresses with regard to the methods of the census are summed up by Korosi in his project for a census of the world, published in 1881.

The first of these conclusions was that the census should be taken every ten years in the month of December, and in those years the number of which terminates with zero; recommending, however, that intermediary censuses should be taken at the discretion of different governments.

The taking of the census at the end of December has the advantage that a relatively greater number of population are in their own homes than at any other time, and that it corresponds to the termination of the calendar year, at which date many State and municipal reports terminate, so that all the figures being for one date are readily comparable with each other. For a very large part of this country it would be quite as easy to take the census at the end of December as on the first of June; but there are some sections in which attempts to take the census in the midst of cold and rainy weather, for a thinly scattered population, would be made under very great difficulties.

In a paper read at the British Health Congress in May, 1889, Sir Edwin Chadwick urges the desirability of an annual census for the improvement of public administration, referring to the fact that "in commerce and in manufactures, as in every large company, there is an annual stock-taking, and upon that stock-taking a report and declaration of the results are made to the stockholders. But what is the state of the political administration which has only attained to a stock-taking of the living people, of the healthy and the weakly, on whom the power and prosperity of the country depend, which is only attempted every ten years, and is only completed in three years, leaving the numbers, meanwhile, to be got at by estimates, necessarily erroneous—often widely erroneous. An annual census of the more numerous animal stock has been lately striven for and attained, and is worked out by the Agricultural Department of the Board of Trade. It is some forty-five millions of the agricultural stock, whilst the population of England and Wales is about twenty-eight millions. Other nations, as France

and the United States, have halved the inconvenience of the stock-taking of the human population."

Sir Edwin is in error in this last statement, for the United States Census is taken but once in ten years, and but a few States have thus far taken the intermediate census, which would make the enumeration come at quinquennial periods.

In the law providing for the last census an attempt was made to induce the States to take this intermediate census, by an offer that the United States should pay one-half of the amount paid to all supervisors and actual enumerators in the State, increased by one-half of the percentage of gain in population in the State, provided that the schedules used should be similar to those used in the census of the United States, and that a full copy of all schedules returned, and reports made, shall be deposited with the Secretary of the Interior on or before the first of September following.

This proviso, however, has not had the effect designed, probably for the reasons set forth in the report on the census of Michigan for 1884, namely, that at least two months are required to make the enumeration, transmit the blanks to the Secretary of State, and arrange them, which leaves but one month (August) for correction of errors and for making the full copy of the schedules. As applied to the State of Michigan, this would require a thousand or twelve hundred clerks for twenty-five days. The amounts paid the supervisors and enumerators for the United States Census of 1880 were $71,192.06, the one-half of which, increased by one-half percentage of gain, gives $42,401.99 as the amount Michigan would have received from the general government if its census had been taken in conformity with the national census law. But it would have cost from $60,000 to $70,000 to copy the schedules and reports, or from $18,000 to $20,000 more than the amount that would have been received from the national treasury to compensate for the work. A few sets of territorial schedules were sent in, but nothing was done with them.

Sir Edwin, however, urges that even the quinquennial census would be considered insufficient for business purposes in the ordinary affairs of civil life, and refers to the fact that the proposition for an annual census in the European governments was discussed at the International Statistical Congress at the Hague, having been proposed by himself and strongly seconded by Prof. Engel, the head of the Statistical Bureau of Prussia. The matter was appointed to be the subject of discussion at a future Congress; but, in the meantime, the meetings of the Congress were interrupted and the matter has never again come up as a subject for international discussion.

Annual censuses have been tried and abandoned in Canada. Triennial censuses have also been tried, but the general conclusion seems to be in favor of five year periods.

In Austria, Belgium, the greater part of the British Empire, Denmark, Holland, Hungary, Italy, Norway, Sweden, Switzerland, and the United States, the census is decennial.

In the Sandwich Islands a census has been taken every six years since 1853.

In France a census was taken in 1801, 1806, and every five years since 1821.

In Sweden, every five years from 1775 to 1860.

In Germany, every five years since 1866.

In Finland, every five years since 1875.

In Sweden, every three years from 1749 to 1775.

In Baden, every three years from 1818 to 1867.

In Hanover, every three years from 1830 to 1864.

In Prussia, every three years from 1834 to 1867.

In Saxony, every three years from 1834 to 1867.

In Bavaria, every three years from 1834 to 1867.

While in Upper Canada (now the Province of Ontario) a census was taken annually for nineteen years, from 1824 to 1842.

In New Zealand nine censuses have been taken since that of 1861, at intervals of from three to seven years, the last interval being five years.

In Queensland there is also a quinquennial census.

While it is not probable that any agreement for an annual census for the whole country will be made in the near future, either by the United States or by State Governments, it is certainly quite possible that the desirability for more frequent enumerations in the larger towns and cities will soon become so evident as to lead to systematic arrangements for carrying it out. Even now, many cities take what they call a police census, at irregular intervals of from three to five years, for the purpose, mainly, of making rough subdivisions of the voting populations in the form of precincts, and of obtaining information for the purpose of levying taxes, more especially for school purposes. These police censuses, however, relate only to adult males, and upon them are based estimates of the number of the remaining population, which are used by the sanitary officials in computing death-rates. These estimates, however, are almost invariably too high, as is shown by the next State or national census, and, in any case, afford no satisfactory basis for computation as to the number of inhabitants in the different wards of the city, owing to the great variations in the ratio between the voting population and the rest of the people in different classes of society and in different parts of a town.

By the use of proper schedules, somewhat on the English system, left at each house, there should be very little extra expense over that of the ordinary police census in collecting sufficient data with relation to the entire popution of a city, to give accurate data with regard to the vital statistics of the population, and these, combined with a good system of registration of deaths and of marriages, would give the means for a system of vital statistics of a place such as, at present, no city in England or in this country possesses.

The unit of area made use of in furnishing the census data is a very important matter in connection with the statistics of deaths, for, practically, these last must be classified by the same areas. The census units of area have usually been fixed by political considerations with reference to votes, to taxation of various kinds, and to military service. In this country, the smaller unit is usually the county or city, or, in some cases, the wards of a city, the larger units being the State or Territory.

For the purposes of vital and medical statistics, such units of area are often of little or no importance, because the boundaries of wards, of cities, of counties, and of States are not fixed with reference to the peculiarities of topography, drainage, character of habitations, or of the people, which are important factors in the causation of disease and death, and which, therefore, require study, with the object of properly dividing the population for purposes of representative government. Moreover,

in the publications of the several censuses prior to that of 1880, the population of the smaller areas, such as cities and counties, was, as a rule, given with distinction of sex and of color only, while the most important factor in vital and medical statistics, namely, age, was stated in detail for States only. In the census of 1880 an important advance was made in this respect, for in connection with the mortality statistics the population of each county having 10,000 inhabitants and upward, and of the large cities, was given with distinction of age under one year and under five years for each sex of each color.

In the next census the population by counties will be given with still further distinctions of age; it will be given with distinction of age under one year and under five years for wards, and in about a dozen of the largest cities it is proposed to select certain areas which present peculiarities as to topographical features, or as to the character of the people inhabiting them, and to give the population of these by age, sex, and color, as a basis for the study of the death-rates of these localities. In a large city like New York, even the ward is usually too large to be a satisfactory unit of area for statistical study, since one ward will contain some of the best and some of the worst habitations, and include classes of people having very different means, habits, and death-rates.

In the annual report of the city of Brussels the population, births, and deaths are given by streets, courts, and alleys. This does very well for Brussels, where the streets are short; but in our large cities, regularly laid out in rectangles, where the streets are miles in length, and traverse localities differing widely in topography and in character of population, it does not seem to be applicable.

The data required on individual certificates of death should correspond to those required on the schedules of living population, and, in addition, should give the cause of death.

The death-rate, or mortality, is the ratio between quantity of life and loss of life. It refers to a definite unit of time, viz., one year's life of one person, and the quantity of life is the sum of the time lived by each of the population expressed in years. Two persons living six months each, or twelve persons living one month each, have one year of life. If the population is assumed to be stationary, that is, one in which the births and deaths and the emigration and immigration are exactly equal to each other, and similarly distributed throughout the year, then the number of the population multiplied into the time under consideration, expressed in years and fractions of years, gives the quantity of life.

A population usually, however, increases in geometrical progression, and in such case we must find, by means of a well known formula, the mean population of the period, which will be less than the arithmetical mean of the populations at the beginning and end of the period, and greater than the population living in the middle of the period; but the differences are small, and, in most cases, either figure may be employed.

For example, suppose we wish to calculate the death-rate of New York City for the year 1888. By the censuses of 1870 and 1880, we know the population on June 1st in each of those years. From these we determine the ratio of increase, and thence the population on January 1 and December 31, 1888, from which the mean population can be computed, or the arithmetical mean be taken. Or we can compute directly the population on July 1, 1888, and it makes little difference as to probable accuracy which of these three estimates of population we take.

You will perceive that this method rests on the assumption that the ratio of increase of a locality during ten years as determined by the census, continues unchanged and uniform thereafter. This is hardly ever true, and, for many localities in the United States, and especially in the rapidly growing West, it is very far from the truth, which is a strong argument for more frequent counts.

Various methods are used by statisticians to correct the estimates of population made for a city at periods other than during a census year, as determined by a formula of arithmetical or geometrical proportion, among which may be mentioned the use of the ratios supposed to exist between the number of houses or the number of voters, or the number of school children and the total population. The number of houses is determined from tax records or by a special count; the number of voters by registration lists or by a police census; the number of school children by a special census; and the ratio which these bear to the whole population is guessed at, or is calculated from the data obtained at the last census.

The most useful and reliable of these methods is the use of the average numbers of presumed occupants of inhabited houses, the other ratios being of very little value.

This has been very well shown by Dr. Russell, Medical Officer of Health at Glasgow.[1] In the case of Glasgow, he considers the estimate of the population based on the number of inhabited houses and the estimate based on preceding censuses, and finds that both methods give a population above the actually existing one as shown by the next census. The error in the estimates based on the previous census was due to a change in rate of growth. The error in that based on the number of houses was due partly to incorrect data of the number of inhabited houses, and partly to an error in calculation.

In the United States Census of 1870 the number of persons in a dwelling in the large cities varied from 14.07 in New York to 5.20 in Toledo, these differences being due, to a considerable extent, to the counting of large tenement houses containing numerous families as a single dwelling.

If we take the data by families, we find in the same census that the largest number of persons to a family was in Kansas City, the number being 5.78, while in New York it was but 5.07.

In 1880 the proportion of persons to a dwelling ranged from 16.37 in New York to 4.68 in Memphis. In Baltimore it was 6.54; in Boston 8.26; in Philadelphia 5.79. The number of persons to a family was: in Baltimore 5.8; in Memphis 4.23; in New York 4.96; in Philadelphia 5.13.

It is evident that no value can be placed on estimates of population of a city based upon the number of habitations it contains, if a cottage and a tenement house are to be equally reckoned as a habitation.

Estimates of the population based on police censuses, on the number of school children, or on city directories,

[1] The Decennial Census as a Basis for Statistics in Intervening Years. Glasgow, 1881.

are of very little use, being, in almost every case, in excess of the truth. No general rules can be laid down for the estimation of the population of a city at a period between two censuses. It is a special problem in each case, for the solution of which there are needed an acquaintance with the locality, to be sure that boundaries have not changed, the information which can be obtained from special local censuses, from the number of inhabited buildings, from migration statistics, etc., all of which must be applied to the data furnished by the last general enumeration of the people, which, in any case, must be resorted to for the ratios which are to be used.

It is to be observed that municipal statisticians and registrars rarely make use of the means afforded by a general census to correct the figures of population; and, therefore, rarely alter death-rates which they have given for the years intervening since the last census; yet it is highly desirable that this should be done in order to prevent the use of misleading figures. For example, in Chicago, in 1873, the registrar estimated the population of the city at 400,000, and deducted therefrom the death-rate for the year as 23.89. Comparing the populations in 1870 and in 1880, it is apparent that the true population was about 350,000, giving a death-rate of over 27.

In Boston, in 1876, the estimated population at the time was 352,758, giving a death-rate of 23.39, while the true population, as shown by comparison of census data, was about 313,000, giving a death-rate of over 26.

The shorter the period for which a death-rate is given the greater is the liability to error. The ordinary forms of weekly death-rates reported for large cities are annual death-rates—that is, they represent what would be the annual death-rate if the proportion of deaths to the population for the week continued for one year. If, for example, a town having a population of 100,000 reports as its weekly death-rate for a given week 25 per 1000, this does not mean that during the week there occurred 2500 deaths, but it means that if the population and number of deaths each week continued the same during the year, 2500 deaths would have occurred in the course of the year, or that for the week in question the number of deaths was 2000 divided by 52.17747. A weekly death-rate is useful to show where the greatest variations have been in the year's mortality, but it is no indication of the health of a town for a particular week, and it is useless as a means of comparison of the healthfulness of one town with that of another. This is largely due to the law of probable deviation or error in mortality statistics in relation to the number of instances used as data without reference to their accuracy. This law of probable error in relation to number of data is an exceedingly important one to be kept in view in all statistical inquiries, and especially in those relating to vital and medical statistics.

For example, suppose that in a village of 1000 inhabitants there occur 25 deaths in the course of a year, and in a neighboring city of 10,000 inhabitants there were 200 deaths in the same time. What is the probability that the death-rate of 25 per 1000 for the first and 20 per 1000 for the second, indicate the relative healthfulness of the two places, supposing the data to be perfectly accurate, and that we have no other information in regard to them than that stated?

Or suppose that during one year, in a population of 100,000 persons there occur 2000 deaths, what is the probability that in a second group of 100,000 persons under the same circumstances, the number of deaths in a year will not be less than 1950 nor more than 2050?

The answers to such questions as these are furnished by formulæ derived from the mathematical laws of probability. The simplest of all these formulæ, and the one most practically useful for rough and ready calculations in cases such as that given in the first example, is, that the mean or probable error is equal to the square root of the number dead. By this rule the probable error for the village of 1000 inhabitants for 25 deaths would be 5, and, therefore, the mortality in this place might vary between 20 and 30 per thousand, without any certain indications of variation in the sanitary conditions of the place. In the city of 10,000 inhabitants with 200 deaths, the probable error is a little less than 15, the variation between 185 and 215 corresponding to an average mortality of 18.5 to 21.5 per thousand, being a probable variation of only 3 per thousand instead of 10, as in the first case. It is very clear, then, that in comparing these two mortality rates, no positive conclusion can be drawn as to the relative healthfulness of the two cities, seeing that the probable variations overlap, as it were, for the rates ranging from 20 to 21.5.

The formula that the probable variation in the number of deaths is the square root of that number gives results that are somewhat too great, being merely an approximation to the true formula, which is of much more general application, but requiring too much computation for general use. I shall refer to this matter of the law of error in more detail in speaking of medical statistics. For the present, I only call your attention to the fact that it is necessary to bear in mind the absolute numbers as well as the percentages, and that, therefore, statements of ratios only are insufficient for definite conclusions.

What is a fair or normal death-rate? Taking an average healthy rural district in the United States where there is little migration the annual gross death-rate for the whole population will be from 13 to 15 per 1000. In towns of from 10,000 to 15,000 inhabitants, having a good general water supply and proper sewerage, the gross death-rate should not exceed 16 per 1000. In cities of from 20,000 to 100,000 inhabitants it should not exceed 17 per 1000, while in cities of over 100,000 inhabitants it should not exceed 19 per 1000. The great causes of high death-rates are poverty, overcrowding, intemperance, excess in heat and cold with moisture, foul air, bad food, impure water, uncleanness, contagion, ignorance, etc.

The statistics of births are of much importance in vital statistics because of the influence of the birth-rate upon the sex and age distribution of the population. Unfortunately, in this country, as I have already explained, hardly any locality possesses such an enforced system of registration of births as to permit of an annual calculation of birth-rates and their comparison with the deaths of children under one year of age, or those born within the year. We can only obtain this data for any large extent of country by referring to the census records.

The usual method of indicating the birth-rate is by giving it as the proportion per 1000 of births to the population of all ages; but a much better and more satis-

factory mode of computation is to calculate the number of births to the number of women between the ages of 15 and 50 or 55, living in the community referred to.

The general subject of birth-rates is interesting chiefly, in relation to social statistics, to the fecundity or rate of increase of population or of people of different races; but in relation to mortality statistics it may also become an important factor in the calculations.

There has been from time to time some controversy between statisticians and health officers with regard to the influence of birth-rates upon death-rates, or as to the precise relations which exist between the two. As the death-rates of infants are much greater than those of the population at higher ages, it has been claimed by some that where there is a high birth-rate there is also a high death-rate; but it cannot be said that this will invariably be the case, or that the one is directly the cause of the other, except in certain cases for a comparatively short series of years.

Starting with a population, if possible, numbering in all 308,378, supposing that there are no migrations, and taking the death-rates for each group of ages to be shown by the New York life table, we have calculated the progress of such a population for 100 successive years under two suppositions, the first for a constant birth-rate of 35 per 1000, and the second for a constant birth-rate of 45 per 1000. The death-rate at the commencement was 32.42 per 1000. Under the influence of a constant birth-rate of 35 per 1000 this death-rate increases during the first period of five years to 33.17, and after that steadily sinks until at the end of fifty-five years it becomes less than the original death-rate, and at the period of sixty-five years has sunk below it, after which it again rises until at the end of 100 years it has reached the rate of 32.44.

Taking now the same population under the same circumstances at a constant birth-rate of 45 per 1000, we see that the death-rate rises in the first five years to 36, after which it steadily sinks as it did in the former case, until after the lapse of sixty years it is only 32.01, after which it again rises, and, finally, comes down to a point a little below that from which it started. In both these cases, then, it is evident that the high birth-rate for a time produces an increased death-rate, since there are no other circumstances present to account for the change. But after the first five years the proportion of those living at the ages at which there is the lowest mortality has been so much raised as more than to counterbalance the large number living in the first years of life, and, therefore, the death-rate steadily descends.

But to understand the full effect of this change it is also necessary to consider the average age at death. Here it will be found that the average age at death at the commencement being 31.37, decreases to 29.65 under the influence of a constant birth-rate of 35 per 1000, while, with the higher birth-rate of 45 per 1000, it diminishes as the death-rate diminishes to the age of sixty-five, at which period it is only 23.02, after which it again rises as the death-rate increases.

Comparing with this the proportions of the population living at each group of ages at the beginning and end of the 100 years under each of these two different birth-rates, it is found that under the influence of the higher birth-rate the proportion of the population under twenty-five remains high, while after that age it falls below the original figures.

As a rule, high birth-rates occur in cities and in the crowded parts of cities, among the laboring classes of the population where the causes of disease and death in infants are especially prevalent.

On the other hand, it is to be noted that a high death-rate among infants has some tendency to increase the birth-rate, because the interval between childbearing is shortened by the early death of the infant; and in the effort made by poor women to avoid frequent childbearing, a common means is to suckle the infant up to at least two years of age, in order to prolong the interval between pregnancies, which is a practice injurious both to the mother and to the child.

If we had under consideration a community in which there were no migrations, and in which the population neither increased nor diminished, the relations between birth-rate and death-rate and the average duration of life could be expressed by a simple formula in such a way that, given either two of these quantities, we could determine the third. If, for example, in a population of 1000 persons five births and five deaths occur annually, and if we assume that every individual lives to the same mean age, evidently just 200 years would elapse before the whole of the original 1000 would have died. This 200 years would be the mean duration of life; and this would be the case also if deaths occurred at different ages; only in such a case many would die below the mean age, when some would greatly exceed it. This subject has been considered by Dr. J. S. Bristowe,[1] who says that "there can be no difference in the healthiness of two localities, in one of which the death-rate is twice as high as that of the other, provided other conditions are such that in both cases the inhabitants attain the same mean age; or, conversely, supposing different populations to enjoy the same mean duration of life, any differences which may be presented by their respective death-rates are due to other circumstances than differences of health." He also says that the average duration of life can be determined by the birth-rate and death-rate taken together, but not from the death-rate alone. If he means by healthiness, mean duration of life, this statement is equivalent to saying that where the mean duration of life is the same it will be alike, an indisputable proposition, though not a very instructive one. But if by healthiness is meant the sum of the conditions of the locality as to altitude, drainage, cleanliness, etc., which tend to lessen or increase deaths in the people living in it, then the statement is incorrect, for it does not take into account age, sex, or race distribution, occupations or migrations. Setting the question of migrations entirely aside, it is perfectly possible that two populations attaining the same mean age, and having the same death-rate, may live in two localities, one of which is decidedly healthier than the other, so that if the two communities exchanged habitations, a marked difference in the death-rates and mean age at death would result.

Putting aside all these purely speculative considerations with regard to what might happen in a stationary population, where there is no migration, let us see what the significance of death-rates is in our cities and rural

[1] "On the Mutual Relations of the Birth-rate and Death-rate." St. Thomas's Hospital Reports, New Series, vol. vii. p. 245, London, 1876.

districts as they now exist. We wish to know how much of the death-rate is due to peculiarities in the character and occupation of the population itself, and how much to peculiarities in the locality, and for each of these classes we wish to know how much is necessary and unavoidable, and how much is due to causes which may be modified or done away with. Precise knowledge on these points we can never have, but we can obtain a sufficient degree of probability to guide our action in the premises.

If we wish to study carefully the influence exerted upon health and life by race characteristics, by residence in a given locality, by marriage, occupation, social standing, etc., we must have the means of comparing results given in different localities, or in the same locality at different times, or for different races, occupations, etc., under like circumstances.

To accomplish this we must, as far as possible, estimate the influence of other circumstances not connected with the particular point which we are investigating, but which, notwithstanding, exercise a powerful influence upon sickness and death-rates, and of these, the two most important influences are those which differences in proportion of sexes and ages of the population to be compared exert.

The means recognized as best calculated to eliminate the influence of sex and age by, as it were, reducing the population to one uniform scale in these respects, is by calculating the expectation of life at each age for all the several conditions of locality, occupation, etc., which we wish to investigate; in other words, by the preparation of what are known as life tables. A life table shows what would be the tendency, or liability, to death at each age in a population in which there is no migration, and in which the births and deaths just equal each other, if such a population were subjected to the same influences tending to produce disease and death as have affected the actual population under consideration, and from which the data are derived. It is, of course, impossible to prepare life tables which shall be strictly accurate and exactly comparable one with another, because it is impossible to obtain strictly accurate data. A life table is intended to answer the question, "Of a million children born, how many of each sex die at each age?" or "What is the time which a man or woman of a given age may be expected to live?" A strictly accurate answer to this question could be given only if we knew the precise dates of birth and death of each of a million of children born under the circumstances we are investigating; and, strictly speaking, this million of children should all be born on the same day. Notwithstanding, by using large masses of data which are more or less attainable, and by applying certain well-known corrections, the individual errors tend to neutralize each other, and we can prepare tables which will be quite accurate enough for purposes of comparison.

A vast amount of labor has been expended upon, and study given to this subject; for immense business interests and important points in the jurisprudence of inheritance depend upon the existence and accuracy of these tables. Hundreds of millions of dollars have been, and now are, invested in life insurance on the faith that certain life tables truly represent the average course and duration of the life of a particular class of the community —and the result of more than a hundred years of experience has been applied to their correction under the powerful stimulus of urgent need, from a pecuniary point of view, to have them as accurate and reliable as possible.

Probably the earliest form of such a table known is that given in the Pandects of Justinian by Domitius Ulpianus, commonly known as Ulpian, a distinguished lawyer, who was the secretary of Alexander Severus, and who wrote in the beginning of the third century of the Christian era. This table is found in an extract from his writings given in the Pandects in a treatise by Æmilius Macer. Ulpian states that in Rome registers of Roman citizens, including the data of age, sex, and death, were kept by the city from the time of Servius Tullius to that of Justinian, including a period of 1000 years. From these data, which applied to the more prosperous class, Ulpian gives a scale for estimating the purchase value of communities according to the different value of life at different ages, and sums it up in saying that it is usual to compute thirty years maintenance for all those under thirty years of age, and that for all over that age as many years are allowed as they lack of sixty.

In connection with the question of annuities, Ulpian gives a scale for estimating them, which is apparently the probable length of life of persons of the ages named.

The first life-table of modern times was constructed in 1692 by Dr. Halley from the registers of the city of Breslau for five years, and was printed in the *Philosophical Transactions* in 1693. The data for this and for other similar tables constructed in the 18th century were too imperfect to permit of good results, and the first life-table which was selected as of sufficient accuracy for business purposes is what is known as the Carlisle life-table, compiled by Mr. Milne in the early part of the present century.

We now have a considerable number of life-tables applicable to the special classes of those who are likely to insure their lives, derived from the experience of a large number of insurance companies in Europe and in this country; and also a number of life-tables derived from the data of the whole miscellaneous population of the country for England, France, Germany, Sweden, and for certain parts of this country.

In order to prepare a life-table for a given locality or occupation we must know the number of persons living at each year of age, and the number of deaths at each age which have occurred among these persons for one or more years. We assume that deaths have occurred at regular intervals during the year for each age, and proceed to compute the number of persons at each age who were living in the middle of the period for which the deaths are registered.

In using census data, however, we cannot directly compare the deaths at each single year of age with the number reported by the census as living at that age, because of the strong tendency of the average man or woman to report ages either of the living or the dead, but especially the former, in numbers which are multiples of ten or five, or in so-called round numbers.

An examination of the census of Massachusetts and New Jersey for 1880 will show that the death-rates calculated from it would indicate that the mortality at 25, 30 and 40 is much less than at the ages immediately preceding and following them. Thus, it would appear that in Massachusetts the mortality of white males at the age of 50 is only 9.42 per 1000, while at the age of 49 it is

16.11, and at the age of 51 it is 19.16 per 1000. There is, however, no such abrupt change in the law of mortality at the age of 50 as these figures would indicate, and it is necessary, therefore, to make corrections for this source of error. It is true that the error in the number of deaths reported at the even decades is usually in the same direction as the error in the number of the living population, and therefore tends to neutralize the error in the computation of ratios; but this is not always the case, and the error is almost invariably in the direction of making the mortality at the even decades too low, and for the adjacent years too high.

The error in question may be corrected in distributing the excess reported at the even decades to the adjacent years, by aid of the calculus; but the easier and usual way is to calculate the mortality rates by groups of ages, including five or ten years in each group—that is, for the periods 20–24, 25–30, 30–34, etc., or for the periods 20–29, 30–39, etc., or for the periods 25–34, 35–44, etc. Of the three modes of grouping, the last is the best, because it properly distributes the excess about the even decades, which is the greatest.

If we take the group of ages 20–29, 30–39, etc., the whole excess for the year 30 is distributed in the succeeding years, whereas a part of it really belongs to the years in the preceding group. This objection applies to a much less degree to the grouping of 35–44, 45–54, etc., for the excess at 45 is much less than at either 30 or 40; still it is well to remember that approximate life tables, calculated as I shall indicate, give for this reason somewhat less than the true mortality, and somewhat too high an expectation of life for adult ages.

This source of error affects all mortality statistics derived from the results of a national census, and from the ordinary system of registration, but it does not affect mortality statistics derived from the records of life insurance companies, in which it may be presumed that the ages of both the living and decedents are accurately stated.

I do not propose to describe the methods of constructing a life table. To make one sufficiently accurate to be used for the purposes of life insurance requires elaborate calculations and corrections, and the use of complicated mathematical formulæ; but the construction of an approximate life table in which no attempt is made to secure regular gradation is a comparatively easy matter, and has been fully described by Mr. N. H. Humphreys in a paper in the *Journal of the Statistical Society* for 1883, which method was made use of in calculating the approximate tables given in the mortality statistics of the last census.

ORIGINAL ARTICLES.

PURULENT PERITONITIS, WITH PERFORATION OF THE APPENDIX VERMIFORMIS.[1]

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THERE was admitted to the Pennsylvania Hospital October 19, 1889, a woman, who gave the following history: She was twenty-three years of age, single, and was born in England. She had been three months in America, and six weeks ago had a miscarriage in the Preston Retreat. She got up and went to work too soon, she thought. Three days before her admission, after having been exposed to cold, she was taken ill. Upon admission the pulse was small and rapid (146 per minute) and her temperature 101⅘°; there was severe abdominal pain, the belly was swollen and tympanitic, and induration was present in the right iliac fossa over the region of the cæcum. There was frequent vomiting, and she said she had had diarrhœa, though while in the hospital she had but one movement. She also declared that she was passing flatus freely, and even in great amount. It was easy to make a diagnosis of peritonitis, and it was thought likely that its origin had been in the reproductive organs, in view of the fact that there had been a miscarriage so recently. As there seemed to be no obstruction of the bowel, operative interference was thought, for the time at least, entirely uncalled for. She was ordered liquid diet and one-quarter grain of opium and one-twelfth grain of extract of belladonna every two hours. Fifteen hours after her admission she died in collapse.

At the autopsy, made twenty-four hours after death, the following was found: Upon opening the abdominal cavity a large quantity of offensive yellowish pus containing flakes gushed out. The quantity was estimated at two pints. The tissues and organs were everywhere glued together by fresh yellowish lymph, the adhesions being quite firm though not difficult to tear. The under surface of the liver was adherent to the stomach, the stomach to the colon, the colon to the small intestine, the various turns of the small intestine were matted together, and all was covered by the omentum, which was everywhere more or less adherent. The appendix vermiformis was as large as the little finger, being much dilated, and was fixed quite firmly by fresh adhesions, which were easily separated, to the inner surface of the *os ilium*, and near its proximal end contained a mass of inspissated material about half an inch in length by one-third in diameter, and pointed at one end; it had the shape of an ordinary conical rifle ball. Between the position where the concretion lay and the outlet of the appendix into the cæcum was a perforation about a quarter of an inch in diameter and circular in form. The inner surface of the intestines was here and there much injected, and in some areas looked as if there had been actual inflammation of the mucous coat, which was intensely red and thickened at such positions. The uterus was slightly increased in size, and its suspensory ligaments were opaque and thickened. The liver presented upon its surface several broad yellowish areas, and upon section it was found that the same yellow discoloration penetrated into the substance to the depth of about one-quarter of an inch. The other organs presented no noticeable abnormal conditions.

It is sad to contemplate the shortcomings of the application of the science of medicine in this case, for certainly the disease was one which might have

[1] Read before the Philadelphia County Medical Society, Nov. 13, 1889.

been cured if it could have been recognized in its early stage, and surgical operation undertaken before it had produced results incompatible with life. It is of little avail, however, and would ill become us as physicians to sit down and moan over the imperfections of our art; rather must we hurry on to improve our means of dealing with such conditions, and of all the directions in which we may turn our efforts for improvement, there is none which calls so loudly for study as diagnosis. The most serious difficulty that besets us in our management of these cases of peritonitis is the uncertainty of diagnosis, and whether surgeons or physicians we must alike struggle to increase the degree of precision with which we may diagnosticate such conditions. I say this with full knowledge of the great difficulties that are to be encountered from the standpoint of treatment and of the somewhat radical differences of opinion that exist in regard to treatment in some particulars, for, unless a diagnosis can be made, treatment has almost no existence, and certainly is of little avail.

In considering my case, as may now be done in retrospect, I am quite satisfied that under the circumstances nothing better could have been done for the patient.

The error of supposing that the peritonitis, which it was easy to recognize, had its origin in the reproductive organs, though the objective evidences which were present—induration over the region of the cæcum and its absence over the uterine region—pointed in a contrary direction, was natural enough, in view of the fact that the woman had had a miscarriage only six weeks before, and that none of the well-recognized signs of obstruction of the bowel were present—fecal vomiting, constipation, and interference with the passage of wind, being all conspicious by their absence. My decision to wait twenty-four hours at least before calling for surgical aid was also, I think, proper; for, as there were no signs of obstruction, I would have been unwilling to countenance immediate operation, nor do I think the surgeons would under the circumstances have wished to undertake it. This determination to wait was further justified by what was found at the autopsy; that the disease, though arising in the appendix, had, when the patient came to the hospital, progressed so far in causing the various abdominal tissues and organs to adhere to each other, and had already so nearly caused the death of the patient, that no operation would have been of any avail. Finally, with regard to the best measures for relief after it had been decided that at the moment no operation was desirable. About a year ago I read before this Society asshort paper upon the treatment of peritonitis (THE MEDICAL NEWS, December 8, 1888), in which I advocated the use of opium and belladonna in moderate doses in such cases as did not seem to require surgical treatment. From the views then expressed I have as yet seen no reason to recede, and the case the history of which I have recounted seems to me an unusually striking instance of the kind in which the treatment with salines not only could by no possibility have been beneficial, but must have been positively injurious. In a woman without obstruction of the bowel, having diarrhœa and passing wind freely, and, as the autopsy showed, having a large amount of pus in the abdominal cavity, and many adhesions, the utmost that the most ardent advocate of saline treatment could claim would be that it might have relieved by locally depleting the congested blood-vessels; but, surely, this must have been more than offset by the weakening effect of the increased diarrhœa, the increased pain and general irritation produced by the already overtaxed bowel being thrown into violent activity. If acute peritonitis of this character is best treated by large doses of saline purgatives it is the only acute inflammation existing which requires such treatment, for it is universally agreed that elsewhere they call for rest and soothing measures. On the other hand, I do not wish to be understood as altogether opposed to the saline treatment, for in properly selected cases requiring surgical aid I should be quite willing to accept it as a fact that the surgeon's knowledge was paramount, and to use salines.

In conclusion I wish to add that the case has in one particular been very interesting from a pathological standpoint, as an illustration of the wonderfully migratory habits of inflammation—a fact with which I am constantly struck. Here was an inflammation which had its origin in the irritation of the inner coat of the appendix by a concretion; the effect was ulceration and inflammation of all the coats, and perforation at the point of pressure; next came general peritonitis, and this by extension, the process of inflammation attacking contiguous tissues, set up in spots a true enteritis, for microscopical examination showed the presence of extensive inflammation of the entire thickness of the small intestine in pieces studied.

A SUGGESTION AS TO THE ACTION OF OLIVE OR COTTON-SEED OIL IN GALL-STONE COLIC:

Observations on the Use of the Oils and Reports of Cases.[1]

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WITHIN the past three years the old plan of treating gall-stone colic with large doses of olive oil has

[1] Much of the oil sold as olive oil is really refined cotton-seed oil. The difference in chemical composition and properties is not very great. Olive oil contains a rather larger proportion of the more fluid fat, olein. In one of my cases olive oil was prescribed; in the other cotton-seed.

come much in vogue, and many communications have appeared in the journals at home and abroad as to its efficiency, but no adequate explanation has yet been offered as to its *modus operandi*. The lack of a wherefore might seem a matter of slight moment, in view of its efficiency were it not that, because no plausible theory of the action of the oil has been suggested, the method has not only failed to receive recognition by therapeutists, but has been subjected to much adverse criticism, efforts having been made to bring it into disrepute, the supposed success of the oil being attributed to errors in diagnosis. This last is perhaps due to the fact that in none of the reported cases in which the oil treatment was used has it been stated that calculi were found which by analysis were shown to be true gall-stones, it either being carelessly taken for granted that the concretions passed were of this nature, or it was stated that masses of soap alone were met with.

Excluding the rare instances in which entozoa mislead, it is unquestionably true that to establish a diagnosis of hepatic colic due to the passage of gall-stones or of inspissated bile, the finding of concretions in the feces is not essential: certain unmistakable subjective and objective symptoms determining the matter very readily. But where these are not detailed, and the ability of the reporter both to apprehend the facts and to make proper deductions therefrom is unknown, an element of doubt as to the verity of the diagnosis is present which renders the conclusions reached valueless. Perhaps for this reason the oil treatment of gall-stone colic has until lately received such scant recognition.

Although at first I was altogether sceptical of a method which seemed founded on no sound theoretical basis whatever, and very naturally viewed with suspicion reports emanating from observers who made the extraordinary blunder of mistaking easily recognizable lumps of soap for gall-stones, their physical appearances in the quantities passed when large doses of oil are taken, being such as scarcely to mislead even the most careless,[1] communications favorable to the treatment continuing to appear, some emanating from known reliable sources, induced me, somewhat less than a year ago, to look upon it with favor and to search for an explanation of the efficiency of the oil. On giving the matter a little consideration then, it occurred to me that if the treatment were efficient the effects attributed to the oil were more likely due to a product of its chemical decomposition, glycerin.

That oils and fats are decomposed in the duodenum into their fatty acids and glycerin there seems no doubt, and this decomposition is probably effected by the pancreatic juice through the action of a fat-splitting ferment, steapsin, as was shown by Bernard. It is now well known that the ingestion of a large quantity of oil is succeeded by the presence in the feces of innumerable masses of soap, the base of which is principally sodium,[1] formed no doubt by the union of the fat acids with the alkali of the pancreatic juice and the bile.[2] It would appear not improbable that glycerin so liberated exerts in the duodenum an action similar to that which takes place when it is introduced into the rectum; withdrawing water, and causing hyperæmia and irritation of the afferent nerves of the part with which it comes in contact, thus leading to powerful reflex peristalsis.[3] In addition to the active contraction which would be caused by the presence of glycerin in the duodenum, and which of itself would probably lead to energetic reflex contraction of the gall-bladder, cystic and common bile-ducts, so that concretions lodged therein, sufficiently small to be expelled, would be rapidly passed onward into the bowel, it is not unlikely that the power of diffusion possessed by glycerin would enable it to enter the ductus communis and even the cystic and the hepatic ducts, and, perhaps, reach the gall-bladder and the liver, producing a similar depletion of the vessels there and, reflexly, exciting the muscular fibres of these ducts to contraction. In consequence of this action of glycerin a copious outflow of diluted bile would also probably occur, which would materially assist in expelling the stone. These, I believe, would be the probable results of the introduction of glycerin into the duodenum in a case of gall-stone colic, in which the stone was not too large to pass readily through the duct: its extrusion following and pain ceasing. As I shall subsequently show, by relating two cases in which this treatment was resorted to, oil in large quantity apparently has the power to check hepatic colic by promoting expulsion of the calculus. That this result is brought about neither by a lubrication

[1] It might be well to recall here a fact not generally known, that gall-stones may rarely be perfectly soft and pultaceous, and as susceptible of being kneaded into various shapes as a piece of putty. This form of stone is believed by Harley to be composed of cholesterin in the preliminary stage of crystalline formation. They may easily mislead, resembling, as they often do, dirty-white barleycorns, orange-pips, grapes, etc. See Harley, *Diseases of the Liver*, Amer. edit., pp. 336, 337. A case of this sort is related by Harley, where he mistook two oblong steatomatous concretions, moulded into the shape of lemon seed, for the latter. *Idem*, p. 344.

[1] See report by Wiley in Prentiss's case, MEDICAL NEWS, May 12 and July 28, 1887, and many others scattered through the journals, in which these masses, by analyses, are shown to be saponified oil. In Prentiss's case complete saponification of nearly a pint of oil is stated to have occurred.

[2] The question whether the intestinal canal contains sufficient alkali to unite with a large quantity of fat acids has no bearing on the well-recognized fact that these acids and glycerin are liberated through a decomposition of the oil in the duodenum.

[3] As was suggested by Anacker (Deutsche med. Woch., Sept. 15, 1887), to whom we are indebted for its recent reintroduction into practice as a laxative, by enema.

of the duct in which the stone is lodged nor by a partial solution of the calculus, has been shown by Chaffard, and as these are the only modes of action at all conceivable if oil itself, and not a product of its decomposition, is the efficient agent, it is likely that oil is not. Since no explanation can be offered as to the efficiency of the oil, and it is known that glycerin is formed from it in the duodenum, and since it is probable that glycerin can produce the effects attributed to the oil, it is likely that these are due to glycerin.[1]

Though this suggestion is sufficiently plausible to meet the facts, I should have hesitated to offer it now, unsupported by experimental evidence, were it not that, since it occurred to me, an enforced absence from my work for a number of months has prevented me from carrying out some experiments on dogs, projected last spring, to determine its value. I should not have placed it on record at this time, but that I desire to report without further delay two cases of cholelithiasis recently encountered, about the diagnosis of which there is no uncertainty, and in which oil was resorted to, to relieve the attending hepatic colic. I thought this a fit opportunity to offer my theory of the action of the oil, that the use of what has been an efficient remedy in my hands might be placed on a rational—if yet theoretical—basis.

CASE I.—Mrs. K., aged forty-two, of short stature, weight 185 pounds, had had attacks similar to that for which she sought relief, for six years. These had occurred at intervals of about six months, for the first three and a half years. She was then free from them for two years, but in the previous six months she had had several. The attacks were usually of long duration, and associated with jaundice. The state of cholelithiasis had never been treated, the attacks alone receiving attention from her physician; morphine being freely used hypodermatically and by the mouth. She was not aware of the nature of her ailment and had never been told to search for gall-stones. I was first sent for on September 29th. As I was then absent from the city, Dr. Pottberg called in my place. Her symptoms were those of pronounced gall-stone colic. She had been in great pain for two days. Dr. Pottberg informs me that having once before administered a half pint of olive oil to a case of gall-stone colic, with the result that all pain entirely ceased shortly after without a resort to anodynes, he concluded to use it in this case. After some persuasion the patient was induced to swallow a half pint of oil, no other medication being used, save heat to the hypochondrium. Pain continued very severe for a half hour and then ceased abruptly. A saline purge was taken a few hours after. The feces were not searched for gall-stones, but an inspection of them by Dr. Pottberg the day following the administration of the oil, showed a great quantity of semi-solid lumps which he very naturally looked upon as concretions resulting from the use of the oil.

The patient remained entirely free from pain until the morning of October 3d. An attack then began, as before, with sharp paroxysmal pain in the right hypochondrium; rigors, followed by fever, gastric irritability and flatulence. I first saw her on the afternoon of October 4th. The liver was found to be enlarged and the lower margins painful on pressure, especially in the region of the gall-bladder. The skin was slightly icteroid, but, it was stated, not more so than was usual in the interval of the attacks. A full dose of morphine and atropine was administered, and inhalations of ether resorted to, with digital manipulation of the gall-bladder, followed by hot poultices to the hypochondrium. Heat was also applied to the feet, which were cold. Draughts of hot water containing sodium bicarbonate were taken at intervals. Under this treatment the pain ameliorated after an hour, but soon became intense again, requiring the use of morphine and atropine, hypodermatically, at intervals. On October 6th, jaundice was more decided; stools were pipe-clay colored and urine high-colored. Pain was now very great, and inhalations of ether, manipulation of the gall-bladder and the abdomen seeming to have no effect in promoting extrusion of the stone, she expressed a willingness to have recourse to the oil. She had objected to it at first as, although the former dose had been followed by such prompt cessation of pain, it had been so intensely disagreeable to her, and so difficult to swallow and retain that I had not urged its use. But now, having determined to take it, she did so resolutely, swallowing three quarters of a pint containing a drachm of ether[1] without demurring. The oil was retained, and, to my gratification, pain ceased suddenly and absolutely within three-quarters of an hour. This was the first time I had used oil to relieve gall-stone colic, and though I had faith in the treatment and had formed a theory to account for its efficiency, I still felt no little surprise at the result.

The bowels moved freely, without a purgative, fourteen hours after the oil was taken. I had already begun a careful search for stones, using a sieve consisting of several thicknesses of cotton-netting of a very fine mesh. Every fecal evacuation after the date of my first visit was most thoroughly washed and filtered, and the scrutiny is still as carefully continued by the patient, who has a very intelligent idea of the matter, saving everything for my inspection which at all differs in appearance from the general semblance of fecal matter. The first three passages following the ingestion of the oil contained upward of fifty semi-solid lumps, of various sizes, similar to those passed before. These I had no doubt were concretions of soap. A few were sent to Dr. Leffmann for analysis, who confirmed my

[1] Rosenberg (Fortschritte der Medicin, No. 13, 1889) has recently ascertained an important fact which tends to support my suggestion. He has found, by experiments on dogs with gall-bladder fistulæ, that large doses of olive oil greatly increase the quantity and diminish the consistency of the bile excreted. It would appear inconceivable that olive or cotton-seed oil could have such an effect, save in the manner I have proposed.

[1] The ether was added with the idea of assisting the retention of the oil, and also, of promoting a flow of pancreatic juice.

opinion. In the third passage two gall-stones were found. One, unfortunately, on too rough handling broke into small bits and was lost. It had a similar appearance to the other, though it was a trifle smaller. This latter was about the size of a large pea, somewhat cubical in shape and of a brownish exterior. On light pressure with the finger it crumbled, showing a white, crystalline interior, presumably of pure cholesterin. Sulphuric acid, placed in contact with a small crumb, gave the play of prismatic colors. Another small portion was dissolved in boiling alcohol and the precipitate examined microscopically after evaporation of the spirits: the characteristic rhombic plates of cholesterin were shown. The remainder, quantitatively examined by Dr. Leffman, was found to consist wholly of cholesterin. The patient stated she found what appears to have been another stone, a few days later. It was globular in shape, and about three times the size of the one tested. As it fell into bits on her attempting to rewash it, and as she neglected to save the fragments, I failed to see it. Though some suspicious-looking concretions have been passed since, and brought for my inspection, none have answered to the tests for gall-stones or inspissated bile. The patient has had one or two slight attacks of colic since, but not severe enough to require medical interference. She is on appropriate hygienic and medical treatment to prevent the redevelopment of stones, and to bring about the solution of any remaining.

CASE II.—Mrs. C., aged forty-five, weight 225 pounds, of medium height, presented herself for treatment on October 23d, last. She had had paroxysmal, cramp-like pain in the epigastrium and the right hypochondrium, with flatulence and nausea, for several days. On questioning her, I learned that this was but a very light expression of similar seizures which had affected her for twelve years. The attacks appeared at intervals of from two years to two weeks, and lasted from a few hours to many days. In the past three or four years they had occurred more frequently than formerly, and were of several days' duration. The shorter attacks usually occurred at night. Decided pruritus and jaundice were often present, after the pains had continued for three or four days. She was not aware that she was passing gall-stones, and had never been treated for them so far as she knew.

On examination, I found it difficult to outline distinctly the lower limit of liver dulness, because of the great amount of adipose tissue. It was, however, found to be extended, and the situation of the gall-bladder was exceedingly tender on pressure. Her diet and habits were regulated, and sodium bicarbonate and sulphate prescribed, to be taken in a bitter infusion a half hour before meals. An opiate mixture was ordered, in case the pains became too severe to be borne. She was carefully instructed how to search the passages for biliary concretions.

On the night of October 27th I was summoned to see her in well-developed hepatic colic. The slight pains had continued, with remissions and exacerbations, without growing very severe until the afternoon of that day, when suffering suddenly became so intense that she resorted to dose after dose of the opiate, entirely without relief, as, owing to the great gastric irritability, each was vomited as soon as swallowed. I gave, hypodermatically, a half grain of morphine and $\frac{1}{100}$ of atropine; applied heat to the feet, and afterward to the liver, first attempting digital manipulation of the abdomen in the region of the gall-bladder. Draughts of hot water, containing sodium bicarbonate and essence of peppermint, were also administered. Relief was soon felt, after which she fell into a broken sleep, to be thoroughly awakened, in the course of two hours, by the pains recurring as severe as before. Another half grain of morphine with $\frac{1}{100}$ of atropine was taken—this time by the mouth. This failing to give ease, and it being daylight, she sent for some cotton-seed oil, a full dose of which I had told her might tend to check the pains, should other remedies fail. I saw her at eleven that morning: she was then suffering very acutely. She had taken seven fluid-ounces of the oil at five, but had vomited it at once after it was swallowed. I persuaded her to take ten fluid-ounces while I was with her, adding a teaspoonful of ether to the dose, and administering a tablespoonful of undiluted brandy immediately after. Emesis did not occur again for an hour: then about half the oil was ejected. Shortly before this, or, as nearly so as could be arrived at, forty-five minutes after taking the oil, the pain markedly diminished, and three hours later ceased entirely, indicating that a stone had been passed into the duodenum. A mercurial purge was taken on cessation of the pain, followed six hours later by two drachms of sodium sulphate in hot water. The bowels moved freely that night and the subsequent day, but no stones were found. Though great tenderness, on pressure, was felt over the lower margin of the liver, and especially above the situation of the gall-bladder, she remained entirely free from unprovoked pain for thirty-six hours. At the end of this period it recurred with great intensity. Feeling that the oil had been instrumental in checking it before, she was quite willing to resort to it again, if there was any chance of it being retained; as she constantly vomited all fluids swallowed, there seemed slight hope of this. It occurred to me that cocaine might aid in its retention. Thinking it worth a trial, I prescribed one-third of a grain, with ten minims each of compound tincture of cardamom and spirits of chloroform, in a tablespoonful of water, to be taken a half hour before the dose (a half pint) of oil; and directed that the oil contain a teaspoonful of ether, as before, and that a swallow of undiluted spirits follow the oil. On visiting her again a few hours later, I was told that vomiting had not occurred until three hours after taking the oil, and only then on attempting to swallow some meat broth. The vomit contained but little oil. It was also stated that the pain, which had been excessive, decidedly diminished within two hours after taking the oil, but did not cease entirely until seven hours later. Morphine was not resorted to during this attack. On its termination, the bowels were kept

relaxed by calomel and sodium sulphate, and the feces searched in the most thorough manner—a forty-mesh wire sieve being used to wash them.

As I had directed that every particle passed of the slightest suspicious nature be saved for my inspection, the soapy concretions and all sorts of foreign substances, such as fruit seed, and concretions evidently formed in the intestines from undigested food, were preserved and carefully tested, chemically and microscopically, when their nature was not apparent by inspection. On the fifth day after cessation of the last attack of colic, a triangular-shaped stone, about the size of a beechnut, was voided. It consisted of cholesterin with a nucleus of mucus and inspissated bile: the tests for these being applied by myself and confirmed by Dr. Leffmann. None has been found since, though the search is still being prosecuted with care.

This calculus, and especially the first one tested in the case of Mrs. K., had the smooth, eroded appearance of a biliary concretion that had undergone partial disintegration within the body. As neither of the cases had been under treatment directed to bring this about for more than a few days, it occurred to me that it might be due to some solvent action of a product of the decomposition of the oil. To determine if either soap or glycerin, when brought in contact with gall-stones, would have the tendency to promote softening of their mass and partial solution of their exterior, I took the halves of a very large and firm cholesterin stone, which I had removed two years before (*post mortem*) from a gall-bladder, and placed one in glycerin and the other in a strong solution of sodium soap, keeping both liquids at a temperature somewhat below the body heat, and noted the result in twelve hours. While the portion suspended in glycerin had remained firm in consistence and was altogether unchanged, that in the soap solution had stained the latter very decidedly and crumbled readily when taken between the fingers. This result is not surprising, when it is considered that soda is the alkali of the bile that tends to maintain cholesterin in solution, and that sodium salts, administered for long periods, in cases of cholelithiasis, have the tendency to promote dissolution of gall-stones and concretions of inspissated bile. This little experiment may, in a measure, account for the failure to find gall-stones where search has been properly made in cases of hepatic colic treated by large doses of oil; for intestinal peristalsis, bringing the recently passed stones constantly into contact with warm solutions of soap, may, in some instances, cause their entire disintegration.

Although I do not claim to have fully demonstrated by the recital of these cases that oil is efficient in checking hepatic colic, since the length of time taken by the passage of a stone into the duodenum can never be exactly determined beforehand, yet I think that the prompt cessation of the attacks in Case I., in which oil was used, and in Case II., their less prompt yet really speedy discontinuance, in comparison with the duration of the last few previous seizures, when ordinary means of relief were tried, indicate, when placed with the accumulating evidence in favor of this treatment, that it is not without utility, and that it is at least worthy of intelligent and extended trial—especially when we reflect that none of the methods ordinarily resorted to have much influence in promoting extrusion of biliary concretions. A stone is probably passed into the bowel, through the action of the contractile muscular fibres of the duct in which it is contained, and the accumulation of liquid bile behind it, urging it along. Opium and belladonna in the full doses necessary to allay the pain of colic, and inhalations of ether and chloroform, tend to impede rather than assist expulsion, by arresting peristalsis in the duct. Opium and belladonna tend to retard, also, by checking the formation and outflow of bile. It is true that dilatation of the duct is necessary for digital manipulation of the gall-bladder to be practised with success, yet few, save the most skilled, are able to apply this procedure properly. The abdomen of gall-stone subjects is usually so adipose that it is difficult to feel and still more to grasp the fundus of the gall-bladder, unless much distended—a condition unlikely to obtain unless a calculus has been some time impacted in the common bile duct.

If the theory suggested as to the action of the oil is correct, the method, as now practised, is open to many sources of failure. Glycerin may not in all cases be formed in sufficient quantity to exert its peculiar effects. A free flow of pancreatic juice is necessary for the splitting up of the oil. The pancreatic juice may not be present in sufficient quantity to act on any amount of oil. Belladonna, or its alkaloid, atropine, much resorted to in hepatic colic, suppresses the pancreatic secretion. If full doses are given before oil is tried, the result will be *nil.* Any fluid taken with the oil may so dilute the glycerin formed in the duodenum that its hygroscopic effects on this part will be prevented. The calibre of the offending calculus may be too great to be influenced by the contractions of the duct and a full outflow of bile, induced by glycerin. If further trial justifies faith in the use of oil in the treatment of gall-stone colic, and my theory of its action is proved by experiment, some of these elements of failure may be removed.

The great objection to the use of oil is the difficulty patients have in swallowing and retaining it in the quantites ordinarily administered. The method practised in Case II. might be tried where considerable gastric irritability exists. Whether a less dose than half a pint will be efficient remains to be determined.

It would, I suppose, be useless to administer glycerin by the mouth with an idea of obtaining the result anticipated from the oil; for, because of the hygroscopic properties of glycerin, on which would depend its action, sufficient fluid would be absorbed from the gastric bloodvessels to render it too dilute to be efficient when it reached the duodenum.

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A CASE OF INTESTINAL OBSTRUCTION FROM APPENDICITIS, FOLLOWED BY PERFORATION OF THE BOWEL ABOVE THE SEAT OF OBSTRUCTION.[1]

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THE usual cause of death in inflammation or ulceration of the vermiform appendix is perforation of the appendix and septic peritonitis. In the case which I am about to report the phenomena which resulted in death were, first, appendicitis, producing local peritonitis and bands of adhesion; second, obstruction of the ileum from these adhesions; and, finally, perforation of the ileum from distention and gangrene above the seat of obstruction.

The patient was a delicate woman, twenty-two years old, with a phthisical family history. She had had no abdominal disease until twenty years of age, when she suffered from an attack of general peritonitis, beginning with right iliac pain and tenderness. She apparently recovered perfectly from this, and continued well until the 17th of last August, when, after excessive ocean bathing, she was seized with pain and tenderness in the right iliac region, accompanied by constipation, nausea, and vomiting. This attack lasted one week, and was relieved by mild saline purgation. Though she left her bed she was not perfectly well, and was never entirely free from pain and tenderness in the region of the cæcum.

Toward the end of September she was again obliged to take to bed: the right iliac pain increased; she had obstinate constipation and paroxysms of pain around the umbilicus. There was no abdominal distention, the abdomen being, on the contrary, rather retracted. Ingestion of food caused abdominal pain, nausea, and vomiting. At first the constipation was overcome with calomel and saline purgatives. It, however, became more difficult to produce a movement, until, by October 10th, there was absolute constipation, or, in other words, complete obstruction. The temperature—which had previously been normal—began to rise a few days before the appearance of complete obstruction, and ranged from 100° to 101.5°. The pulse was about 100. Abdominal distention first appeared when the constipation became absolute. For nine days there was no passage of feces or flatus from the rectum. The other symptoms were gradually progressive. General emaciation rapidly increased, the abdomen became greatly distended, the paroxysms of umbilical pain became more frequent and violent, there being two especially intense attacks—one five days after the beginning of absolute constipation, and the other on the night of the eighth day. The last of these attacks was followed by collapse; vomiting continued with but little intermission, though it never became fecal. She was first seen and operated on by the writer nine days after the beginning of complete obstruction. She was in a condition of collapse, with a quick, feeble pulse, rapid and shallow respiration, and a tense, distended abdomen.

Abdominal section was performed by the median incision. As soon as the peritoneum was opened there escaped through the incision a large quantity of fecal gas, followed by several ounces of very offensive, thin, fecal pus. The intestines were not adherent to the parietes, but all the loops of the small and large intestine in the lower half of the abdomen were united by recent friable adhesions. The pelvis was filled with a large quantity of unhealthy lymph, pus, old and recent blood-clots. In the region of the cæcum the intestinal adhesions were old and strong. One inch from the ileo-cæcal valve the lumen of the ileum was completely closed by a thick mass of inflammatory tissue, which bound the bowel to the posterior wall of the abdomen. The vermiform appendix was imbedded in adhesions; was two inches long, and about one-half inch in diameter. It was hard and rigid, but from the hasty examination made at removal, no sign of perforation was detected. It was unfortunately lost by the nurse. The ileum for twelve inches above the seat of obstruction was hypertrophied, distended, and gangrenous in spots. In a space of ten inches there were two minute perforations on the convex surface of the intestine.

As it was impossible to relieve the obstruction without resection of the intestine, the diseased portion of the ileum—twelve inches in length—was removed. The ileo-cæcal valve and the free end of the ileum were closed by a continuous peritoneal suture. The ileum was then united by lateral approximation with rubber disks to the anterior surface of the cæcum. The abdomen was freely irrigated, and a glass drain was introduced. The patient died a few hours after operation.

I will briefly summarize the points of interest in this case:

The disease began as an appendicitis, probably as a catarrhal inflammation with superficial ulceration. This was followed by local peritonitis, with adhesions around the appendix and cæcum The evidence of this is found in the history of the two previous attacks, which were characterized by right iliac pain and circumscribed tenderness. The adhesions which followed the second attack produced, at first, partial obstruction of the ileum. As the obstruction became more complete the patient began to suffer with paroxysms of umbilical pain, in addition to the ileo-cæcal pain which had characterized his first attacks. Distention and increased pain followed complete obstruction. A few days

[1] Read before the Philadelphia County Medical Society, November 13, 1889.

before death there was probably rupture of an intestinal bloodvessel, and, perhaps, beginning perforation during a violent peristaltic effort to overcome the obstruction. The older clots found in the peritoneum were the results of such a rupture. The perforations discovered at the time of operation probably followed a paroxysm which occurred not many hours before death, for in the exhausted condition of the patient such an accident would cause speedily fatal collapse.

I have reported this case because it adds another to the list so rapidly accumulating which illustrate the dangers that may be avoided by early operation in recognized cases of appendicitis, the operation being not only a curative, but a prophylactic measure.

In pyosalpinx and extra-uterine pregnancy operation was not many years ago deferred until rupture had taken place. We now acknowledge the value of early operation whenever such conditions are recognized. Inflammation of the vermiform appendix is attended with analogous dangers. Most operations hitherto reported have been performed after perforation had occurred, and the patient in many cases was dying of septic peritonitis. Delay is often unavoidable on account of uncertainty of the diagnosis. In other cases operation is postponed because of the delusive hope that the final attack may terminate in temporary recovery as perhaps previous attacks have terminated.

If we recognize, however, that most cases which have a history of recurring attacks of pain and circumscribed tenderness in the right iliac region, with other important though less characteristic symptoms, are due to disease of the vermiform appendix, operation is indicated, either in an interval, while the patient is in fair health, or at the beginning of an acute attack.

I have said very little about the steps of the operation in this case, because the patient died too soon for any determinate result. The rubber disks employed are similar to those which I use for making lateral anastomosis between the ileum and cæcum. Intestinal anastomosis by means of decalcified bone, catgut, or rubber disks, enables us in many cases to avoid the necessity of an artificial anus. The operation is as quickly performed as the formation of an artificial anus, and furnishes an immediate relief to the patient.

THREE CASES OF APPENDICITIS; OPERATION; RECOVERY.[1]

BY J. M. BALDY, M.D.,
OF PHILADELPHIA.

ON a number of former occasions I have taken the opportunity to advance the doctrine of early and prompt surgical procedures, where it seemed likely that the surgeon must interfere eventually. In appendicitis (I use the term advisedly and broadly, for I believe it well proven that the vast majority of inflammatory attacks in this region are such) this doctrine of early operative interference is imperative. In the history of surgery the greatest strides in lowering the mortality and insuring the success of any given operation have been made when the point of operating at a time of election has been reached, and that time of election has been an early one, as a rule. And so it is with the disease under consideration. In the course of progress attention was first directed toward the surgical treatment; then it began to be realized that operations, if they were to prove of benefit, must be done, not as a last resort, when the patient was dying and everything else had been tried and found wanting, but within a certain limit of time, from the third to the sixth day, or at such a time when a certain set of symptoms had declared themselves, these symptoms usually meaning the existence of an abscess. As the truth of these teachings began to dawn on operators, successes following operations began to be reported, until now there are quite a large number on record, many of them in our own city.

In spite of the number of cures by operation, the deaths, both following operation and when no operation has been performed, so far outnumber these, as to leave them as a mere spot on the horizon. At nearly the same time two surgeons, Treves and Senn, illustrious representatives of the progressive elements of England and America, publicly urge another step in advance, a most important one, and support their arguments by illustrative cases. The point chiefly advanced by both is, not to await the time when the patient's life is in extreme danger, but where there have been repeated attacks of inflammation in the right iliac fossa, to elect deliberately a time between these attacks when the patient is in good health and well able to stand the slight shock of the operation. Treves reports the case of a man on whom he did this prophylactic operation, in the *Lancet* for February, 1889. His patient had had three attacks of inflammation, at intervals of about three months, each one more severe than the previous. Treves, finally, to prevent the risks of a fourth attack, opened the abdominal cavity and removed the appendix. This he found tightly bound down with adhesions and under coils of intestine adherent to each other and to the appendix and cæcum. The organ was as thick as a man's thumb and was distended with mucus. It was singularly white in color, a peculiarity I have myself noticed in several cases. Recovery was complete and lasting. In the *Journal of the American Medical Association*, November, 1889, will be found Senn's article. He reports two cases. The first

[1] Read before the Philadelphia County Medical Society, Nov. 13, 1889.

operated upon by himself for five previous attacks of perityphlitis. He found an appendix the size of the thick part of the little finger, but not adherent. On section, after removal, an ulcer one-half inch long by one-quarter inch wide penetrating the mucous membrane of the organ and extending into the muscular tissue, was found. The patient has had no return of the old symptoms. The second case reported by Senn was operated upon by Hoegh, of Minneapolis. There had been at least twelve attacks of inflammation, more or less severe. The abdomen was opened by the advice of Senn, and the appendix, which was adherent, removed. On section, several drops of pus flowed out and two large ulcers penetrating through the mucous membrane were found. Recovery was permanent. Since, and even before, these cases were reported lesser lights were at work, and now there are quite a list of such operations on record. One of these I saw Dr. Bernardy perform on a girl who had had repeated inflammatory attacks. In this case the appendix itself, after being freed from its adhesions, was allowed to remain, it being considered healthy enough to do no harm. The patient made a complete recovery and has remained cured. I have myself, on three occasions, removed the diseased appendix.

During December, 1887, I was called to see Mrs. E., and found her suffering with an attack of peritonitis. She had been in bed much of the time for five weeks. She gave a history of a number of similar attacks during the past few years. An examination of the pelvis disclosed inflammatory disease, and consequently operation was advised. The operation revealed a double chronic salpingitis with general adhesions. To the right Fallopian tube, the vermiform appendix was adherent throughout its entire length, together with a knuckle of small intestine. The adhesions were freed, and after throwing a ligature around the appendix above the diseased part, it was removed. The surfaces denuded by the freeing of adhesions bled freely, and a few silk stitches were put in to bring the peritoneal edges together. The peritoneum was stitched over the stump. The uterine appendages were also removed. Recovery was complete and uneventful. In this case the disease was evidently dependent on the pelvic inflammation. The appendix was perforated in several places, and the whole organ was enlarged to the thickness of one's little finger, and was cheesy and friable.

In November, 1888, I was asked to see Mrs. M., of Trenton, in consultation. She had a history of pain in the right iliac fossa for some years. One month before the present illness she had marked symptoms of extra-uterine pregnancy, with rupture. At the time of my seeing her she was bedridden, and dying of sepsis. An operation disclosed the remains of a ruptured left tubal pregnancy, which was in a state of beginning suppuration. In the right iliac fossa was a mass of adherent intestines, containing in their midst a diseased and universally adherent vermiform appendix. The adhesions were freed, and the appendix found to be as thick as the little finger, cheesy and perforated at two points. It was ligated *en masse*, and cut away close up to the cæcum. The peritoneum was not stitched over the stump. The left side of pelvis was cleared of its diseased tube, and the patient after a prolonged sickness made a complete recovery, and remains to this day a perfectly well woman. No cause was found for the diseased appendix.

During January, 1889, I was called to see Mrs. F. She gave a history of repeated attacks of pain in the right iliac fossa. During the attack in question the pain was worse than ever before. No tumor could be detected, but there was great tenderness on deep palpation. A pelvic examination revealed enlarged, prolapsed, and excessively tender ovaries. I operated by a median incision, being uncertain whether the attacks of pain were not due to ovarian disease. The appendix was found universally adherent under a coil of adherent intestines. The adhesions were freed, the appendix ligated *en masse* close up to the cæcum, and cut away. The peritoneum was not stitched over the stump. Like the other two cases, the appendix was found as thick as one's little finger, or thicker, and was extremely cheesy and friable. No cause was found for the disease. The patient made an uninterrupted recovery, and has since remained well.

In none of these cases was there a mesentery of any length to the appendix.

Here, then, by the prophylatic operation we have a mortality of *nil*. Every case of which I am cognizant has recovered. What a difference between this clean record and that with which we must contrast it! The step will be, no doubt, considered a radical one by many, and there will be raised the usual hue and cry against surgical greed; but until a better record can be shown by any of the older methods, we will proceed on the even tenor of our way, and remove a diseased appendix wherever and whenever it can be found. The world has become reconciled to and looks with complacency on the removal of Fallopian tubes similarly diseased and adherent; and so it will be with the appendix vermiformis. They are even more of a menace to the patient's life than are the cases of salpingitis which we all remove, and the diseases are in many respects similar. It may be argued that these cases were successfully carried through the many former attacks of inflammation, and could be again just as successfully treated without the use of the knife. Granted that they were relieved of former attacks,—but the relief proved in every case to be but temporary; and who is to say where the limit in the number of attacks will be reached, or what one may prove fatal? It certainly is not safe to await pus formation if we can anticipate it; the operations after pus has appeared show sorry results. It was only last winter that this stage was awaited in the

case of a medical student in this city, and the student died.

It will be asked, How can we be certain that the appendix is diseased? To this I can but say that, in my experience, there is no positive, constant sign. The most constant and invariable symptoms that I have observed are pain and tenderness in the right iliac fossa. The symptoms of vomiting, tumor, peculiar drawing up of the knees, etc., are too often absent to be depended upon. If they are present, well and good; they form valuable corroborative signs. But with repeated and persistent attacks of pain and tenderness, of sufficient severity, in the iliac region, nothing else being found to account for them, a diagnosis of pericæcal inflammation is warranted.

HOSPITAL NOTES.

HÆMATURIA.

Abstract of an Original Lecture delivered at the Hospital of the University of Pennsylvania.

BY JAMES TYSON, M.D.,
PROFESSOR OF CLINICAL MEDICINE.

THE patient presented was a girl of twenty years, a domestic, who enjoyed good health until five weeks ago, when she suddenly began to pass bloody urine, with no other apparent symptoms. When brought to the clinic she was anæmic, but this seemed to be rather a result than a cause. In studying a case of hæmaturia, Dr. Tyson said that the first thing to determine is, whether the hemorrhage is from the kidney or the bladder. In this instance he thought it was from the kidney, because of the intimate admixture of the blood and the urine, the absence of clots and of irritation of the bladder.

Supposing it to be from the kidney, it may be due to a variety of causes. Small hemorrhages are found in acute Bright's disease and chyluria. Malaria in this country is quite a common cause. Impacted calculus, tumors of the kidney, injuries and wounds are also frequently the cause. In tropical countries, parasites in the kidney often excite severe and exhausting hemorrhages. Finally, there occur cases of renal hæmaturia which are unaccountable.

In this case the cause could not be acute Bright's disease, because the amount of blood was too large, and there were no other symptoms of that disorder. For the same reason it could not be chyluria. It is not likely to be caused by impacted calculi, because there is no pain and no attacks of nephritic colic. Dr. Tyson thought it more likely to be malaria, but it could not be definitely settled in this case, as a direct history of exposure to malaria was absent, and the blood had not yet been examined for the malarial plasmodium of Laveran. Parasites in the kidney are scarcely to be thought of in this climate. Treatment, of course, must vary with the cause. The patient should always be put to bed. In malarial hæmaturia, quinine freely administered will generally promptly arrest it. In the absence of a definite cause, gallic acid should be the first remedy used, in doses of 15 grains every 3 hours. Dr. Tyson has had success with sub-sulphate of iron in ½ grain doses, which he combines with 1-30 grain of sulphate of strychnine. Among the most valuable remedies are the natural astringent mineral waters, such as Rockbridge Alum Water, the water of the Bedford, Va., alum springs, or of Wallowhatoola alum springs, also of Virginia. He related a case which, having resisted all other treatment, appeared to be relieved by electric baths.

MEDICAL PROGRESS.

Fastening Rubber Bandages without Tapes.—The Swiss correspondent of the *British Medical Journal* suggests the following convenient method of keeping rubber bandages in place on the leg, without the use of inelastic tapes, which, by retarding venous circulation, often frustrate the object for which the bandage is applied. The method in question consists in applying the bandage to within six or eight inches of the end, this free end is then thoroughly moistened with water and slipped under the last two turns of the bandage. According to this authority, if the bandage is well moistened, as described, it will more certainly remain fixed than if tied in the usual manner.

Ointment for Conjunctivitis.—*L' Union Médicale* for October 5, 1889, has the following treatment for inflammations of the eye:

Extract of belladonna .	1 drachm.
Mercurial ointment . .	2½ drachms.

This is to be employed smeared above the orbit in inflammation with great pain and in injuries of the eye. Also where pain is due to a foreign body.

Pilocarpin in Gall-stones.—DR. LEKARSKIE considers pilocarpin almost a specific in the treatment of gall-stones, basing his opinion on the results of treatment in thirty cases. He administers one-eighth grain hypodermatically once or twice daily. The effect on the pruritus of jaundice is immediate, and adds greatly to the comfort of the patient.—*Bulletin Générale de Thérapeutique*, October 15, 1889.

Iodoform in Scrofulosis.—M. BESNER prescribes iodoform to scrofulous children in the following formula:

℞.—Iodoform . . . . . gr. jss.
Mel. despum . . . . . f℥iv.—M.

Of this, a teaspoonful may be given three times daily. He prefers iodoform to the preparations of iodine, from the fact that it may be used for a longer time without interfering with digestion. Iodine in its elementary form he considers more efficient than the iodides, and prescribes the tincture in doses of one drop diluted with boiled milk.—*Revue Générale de Clinique et de Thérapeutique*, October 24, 1889.

Salicylic Acid in the Treatment of Corns.—Moisten the corn with a solution of salicylic acid, and sprinkle over the surface a layer of pure acid. Cover with a small pad of salicylated cotton and oiled silk. This dressing should be renewed every four or five days until the corn is desiccated, when it is easily removed. One or two weeks' treatment is usually sufficient.—*Revue Générale de Clinique et de Thérapeutique*, October 24, 1889.

Chronic Cystitis.—PROF. MOSETIG-MOORHOF advises the following mixture in chronic catarrh of the bladder:

℞.—Iodoform	ʒjss.
Glycerin.	fʒx.
Aq. dest.	fʒijss.
Tragacanth	gr. iv.—M.

A tablespoonful of this is added to one quart of water and injected into the bladder every third day. Improvement is usually noted after three or four injections.—*Wiener medicinische Presse.*

Hypodermatic Injections of Carbolic Acid in Erysipelas.—DR. PAUL SAMTER, of Danzig, in the *Deutsche medicinische Wochenschrift*, enthusiastically advocates the treatment of erysipelas with injections of carbolic acid, believing that, if used early, it prevents suppuration and other complications. Cases uninfluenced by sufficiently large doses of the acid, he believes, are not instances of true erysipelas, however much they may resemble that disease. The drug is administered in the following formula, and freshly prepared for each case:

℞.—Acid. carbol. } Alcohol }	āā ♏ xxij.
Aq. dest.	fʒijss.—M.

Of this, from one-half to one syringeful is injected into the healthy skin about an inch from the border of the erysipelatous area. Without pinching up a fold, the needle is thrust obliquely toward the disease, and the solution deposited in the deeper tissues. If not used upon the face, these injections cause but little pain. The diseased surface should be surrounded with a series of such injections from one to four inches apart. To prevent carbolic acid poisoning, Samter advices that with the above treatment a tablespoonful of a 3 per cent. solution of sulphate of sodium should be administered hourly.—*Therapeutische Monatshefte*, October, 1889.

Dangers of Parenchymatous Injections of Goitre.—At the recent Congress of German Physicians and Naturalists, DR. HEYMANN, of Berlin, reported the case of a woman in whom a goitre had diminished to one-third of its former size after injections of iodine, practised twice a week for four months. After the last injection there were great pain, vomiting, loss of consciousness, contractions, and gradually increasing loss of sensibility and movement in the upper extremities, and death followed two days after with signs of cardiac paralysis. No autopsy was obtained.

The author was of opinion that thrombosis had occurred from penetration of the iodine into a large vein, the clot extending to the external and intracranial branches of the internal jugular vein. Heymann had collected sixteen recorded cases of death after parenchymatous injections. The same result may follow from injection of other drugs, *e.g.*, perchloride of iron and ergotin, etc.—*Journal of Laryngology and Rhinology*, November, 1889.

Reduction of Temperature by a Spray of Water.—DR. PLACZEK, in Virchow's *Archiv*, advocates spraying the entire body as an efficient means of reducing temperature. In an animal with high temperature, he succeeded in reducing the same two degrees by spraying the body with one and a half pints of water at a temperature of from 53° to 59° F., and immediately after with three ounces at 95° F. The after-spraying with water of a higher temperature dilates the capillaries, and this induces a consequent loss of considerable body-heat.

Thus in a tuberculous subject whose evening temperature would at times reach 104° the author reduced the same to normal by using somewhat over one pint of water of from 59° to 66° F. The temperature was with ease kept for four hours at this standpoint and then gradually allowed to rise, but not allowed to reach its former high standing.

Compared to the ordinary method of bathing, this treatment has the advantage of simplicity and comfort, factors not to be disregarded in private practice. The patient simply remains in bed, coverings and shirt are removed, a rubber cloth laid under him, and the *modus operandi* proceeded with. As each application does not require more than twenty-five minutes, it can be repeated several times daily.—*Medical Chronicle*, October, 1889.

Treatment of Fractured Patella.—In a paper read before the Royal Academy of Medicine in Ireland, MR. MYLES, after alluding to the unsatisfactory results in treating fracture of the patella with external mechanical means, to the dangers of wiring even under antiseptics, and to the well-known objections to Malgaigne's hooks, describes his own method of treatment thus:

The limb being cleaned, the skin is drawn well up; a small puncture is then made through the skin on either side with a common bradawl; the upper fragment is cautiously drilled from side to side, the drill being constantly kept accurately parallel to the anterior surface of the fragment; through the drill hole a stout nickel-silver pin is passed, its two ends project on either side of the fragment, its centre is buried in the substance of the fragment. The lower fragment is then similarly drilled and transfixed, the skin having been previously drawn well down.

With the pins now in position a powerful grip on the fragments is secured, and it is astonishing how little force is necessary to overcome the muscular contraction. When the fragments are drawn together and fixed in proper apposition, a gauze tape can be passed under and around them in a figure of 8, as in a harelip operation, or the free ends can be securely wired together.

Corrosive lint, a posterior splint and flannel roller completes the dressing, which is not disturbed for five or six weeks.

The advantages claimed for this method are:

1. That it is effective—that is to say, it will keep the fragments in perfect apposition without the tilting incidental to mechanical appliances.

2. It is free from the danger of septic infection, as the joint is not opened.

3. It is easy and simple, requiring no costly appliances.

Comparing this method with Mr. Treves's modification of Malgaigne's hook, Mr. Myles thus summarizes the disadvantages of the latter:

1. The joint is often perforated above or below.

2. The hooks also tend to tilt forward the lower end of the upper fragments as they press the patella from the front and above.

3. They are painful and cumbersome.—*The Dublin Journal of Medical Science*, November, 1889.

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THE TREATMENT OF RETENTION OF URINE DUE TO ENLARGED PROSTATE.

ENLARGEMENT of the prostate gland, with its consequent disastrous effects upon the urinary bladder, occupies, unfortunately, a prominent place in the long list of diseases that belong to old age, and, until recent years, all that surgical skill could do for it, no matter how severe might be the suffering it caused, was to deal with it in a manner purely palliative. To be sure, in a certain proportion of cases the use of the catheter has yielded results more or less satisfactory, and, of course, there always will be cases in which it will be found a valuable source of relief. On the other hand, instances will be met with in which the catheter, although it may have been for years employed with none but satisfactory effect, all at once becomes a source of irritation, or infection perhaps, and establishes a troublesome cystitis, which eventually is fatal. For such cases the surgeon should possess radical means of cure.

As a step in this direction the operation of perineal incision for drainage of the bladder, which has been a recognized procedure for at least a decade, was introduced. It holds out the promise of certain relief, but not of cure, and in spite of the good results it has yielded, it possesses the disadvantage of being but a temporary measure. Permanent cure is attainable only through an operation which effects the removal of the hypertrophied prostate, or such part of it as is obviously the cause of the patient's disability. Prostatectomy is an accomplishment of modern surgery, in fact, of very recent years, and, as is generally known, there are three methods in which it has been performed—the urethral, the perineal, and the supra-pubic. Against the first two the strong objections of uncertainity and extreme difficulty of performance must be urged, while the last is an essentially simple operation, and presents the great advantage of permitting the operator to learn through the sense of sight, as well as touch, the precise state of affairs he has to deal with.

At the meeting of the British Medical Association, held at Leeds in August last, there took place a most interesting discussion on the subject of "The Treatment of Retention of Urine from Prostatic Enlargement." It appears in the *British Medical Journal* for October 19. The discussion was opened by Mr. A. F. McGill, Surgeon to the Leeds Infirmary, who presented in a very concise manner an admirable and most instructive paper. Confining his attention entirely to those cases of enlarged prostate in which retention is chronic, requiring habitually artificial relief, the speaker made his remarks in a series of propositions, which he himself discussed, and then invited expressions of opinion from the other members of the surgical section of the Association. Mr. McGill's propositions are the following: I. That prostatic enlargements which give rise to urinary symptoms are intra-vesical and not rectal. II. That retention is caused by a valve-like action of intra-vesical prostate, the urethral orifice being closed more or less completely by the contraction of the bladder on its contents. III. That in many cases self-catheterization is the only treatment required. IV. That when the catheter treatment fails, or is unavailable, more radical measures are necessary. V. That this treatment, to be effectual, should (1) for a time thoroughly drain the bladder, and (2) permanently remove the cause of obstruction. VI. That these conditions are best fulfilled by a supra-pubic rather than a urethral or perineal operation. A table comprising twenty-four cases in which the operation had been performed at the Leeds Infirmary accompanies the paper, and from it we learn with surprise of the excellent results obtained. Considering the fact that the patients were all elderly, some very old and feeble men, the mortality of four cases in twenty-four (16.6 per

cent.) must be considered, as Mr. McGill puts it, "lower than might reasonably be expected." Granting, however, that recovery was the result in so large a majority of these cases, so far as the operation itself was concerned, the question still remains to be answered as to what was accomplished in the direction of radical cure. In reply we have from McGill the following figures, excluding from the twenty-four tabulated cases seven in which the operation was undertaken for the removal of stone and the prostatectomy was incidental: four deaths, three resulting immediately from the operation, and one from pneumonia during convalescence; two cases still under observation, and one lost sight of—fourteen in all—ten remaining in which the final result can be stated. "Of these ten, eight have continued well, but one requiring the passage of a catheter, and that only after excessive drinking." The ninth case was one in which it was found impossible to remove the prostate, owing to its extreme hardness, and in which accordingly only supra-pubic drainage was attempted. The tenth case died ten months after the operation, having, however, been relieved for a time. To have afforded permanent relief in eight out of ten cases seems to us a most creditable showing.

We cannot forego alluding to the very graceful manner in which Mr. McGill acknowledges as the first surgeon to perform supra-pubic prostatectomy Dr. Belfield, of Chicago, the date being October, 1886.

The discussion which was elicited by Mr. McGill's paper was scarcely less interesting, and the unmistakable bent of opinion was in favor of radical operation in properly selected cases of prostatic enlargement.

Let us hope that American surgeons, many of whom have already performed supra-pubic prostatectomy, will give it the fair trial that it so richly deserves. Surely, the above results invite it.

PHTHISIS VENTRICULI.

THE efforts which are made to mark the advance in knowledge of diseases by corresponding improvement in their nomenclature are by no means always successful. On the contrary, it often happens that an enlightened pathology at once discards a number of unsuccessful terms, invented to set forth the essential anatomical or clinical characters of a disease, in favor of well-worn popular designations. Terms like influenza and chorea embody no scientific conception of the pathological processes which they designate. On the other hand, they convey no false views of the nature of these diseases.

The growth of knowledge in regard to diseases of the stomach, which has been so conspicuous in the last two decades, has given rise to a number of new nosological terms, several of which have no adequate *raison d'etre.*

Meyer, who suggests the name "phthisis ventriculi" for the process characterized by progressive atrophy of the glands of the stomach, points out, in a recent number of the *Zeitschrift für klinische Medicin*, the defects of several of these terms. The term "atrophy of the stomach" is altogether inappropriate for a condition in which the organ is very frequently much enlarged, and occasionally presents, whether enlarged or not, decided thickening of its walls. "Atrophy of the glands of the gastric mucous membrane" is objectionable, seeing that the term atrophy conveys the idea of persistence of the wasting structures, while in the condition under consideration the glands themselves wholly disappear. Atrophy of the gastric mucous membrane is a designation hardly applicable to a process in which finally a mere layer of connective tissue replaces the mucous membrane.

The German "Schwund der Drüsen der Magenschleimhaut," which is about equivalent to "obliteration of the glands of the mucous membrane of the stomach," well describes a condition which is the terminal stage of chronic inflammation of this membrane. It is, however, too cumbersome for ordinary use; nor are the processes by which this condition is brought about adequately represented by such terms as "gastritis atrophicans" or "catarrhus atrophicans." As a matter of fact, phthisis begins when inflammation and catarrh have run their course. Nor are the gastritis and catarrh which lead to phthisis specific processes, since any form of chronic gastritis can under certain circumstances end in complete wasting of the mucous membrane. It is not easy to estimate the confusion to which this use of this word catarrh, in defiance of its etymology, has given rise.

In support of the term "phthisis ventriculi" Meyer calls attention to the fact that the lesions by which the secretory functions of the stomach are impaired are associated with changes in the muscularis, which also progressively impair its motor function, and with other alterations in the mucous membrane which in a corresponding manner decrease its

power of absorption. Thus the three most important functions of the stomach, secretion, movement, and absorption, are progressively abrogated by changes in its parenchyma. Such parenchymatous changes in other organs, accompanied by wasting of the whole organism, are spoken of as phthisis — for example, phthisis pulmonum. As secondary changes in the organism at large follow these wasting processes in the stomach, the author regards the name "phthisis ventriculi" as a designation eminently appropriate, especially in view of the progressive nature of the disease.

The terminal condition which is marked by complete abolition of the secreting structures is best described by Ewald's term, "gastric anadenie."

REVIEWS.

A TEXT-BOOK OF ANIMAL PHYSIOLOGY, WITH INTRODUCTORY CHAPTERS ON GENERAL BIOLOGY, AND A FULL TREATMENT OF REPRODUCTION. By WESLEY MILLS, M.A., M.D., L.R.C.P. Lond., etc. Illustrated. 8vo., pp. 700.

To anyone who knows of Dr. Mills's thorough work in scientific lines, this book seems to be just what such a student of physiology would write, and to give, as he points out in his preface, an opportunity for the grouping together of certain facts which deal with the study of animal function, and not with human physiology, about which we of course know very little. This has always been a favorite theme of the author, and he has inveighed against the use of the words "human physiology" as applied to text-books for a long time, pointing out that nearly all our knowledge of vital function is obtained from the lower animals. We think, however, that there are two sides to the question, one of which Dr. Mills has overlooked in his attack on the terms commonly employed, namely, that to the vast majority of persons animal physiology is of little interest, and that its entire value depends upon its relation to man. To prevent all fault-finding, the correct title of a physiological text-book should be, perhaps, "Animal Physiology as Applied to Man." So far as the ordinary medical student is concerned, very little embryology is necessary, and yet we find a large part of the book, comparatively speaking, is devoted to this subject, showing once more that the author has placed his stamp upon his work. Still further evidence of the direction of his studies is the consideration of the functions of many of the forms of life far removed from man.

In the article on respiration, we are sorry to note that no notice is taken of Marchwald's work on the nervous mechanism of breathing, particularly in reference to the hypothetical respiratory centres in the spinal cord, in which the author evidently has some belief.

In the article on animal movements, which has of late come to be so important a subject, the work of Mr. Muybridge, which is certainly the most valuable, has also been ignored.

These are some of the faults; let us consider some of the virtues of the book. We find that it is the best one of its character extant, dealing, as it does, in a thoroughly clear manner with every subject which it discusses, and although every part may not seem complete, as a text-book for a student it is assuredly thorough enough. Dr. Mills impresses upon nearly every page the fact that he is a teacher, and apparently recognizes the needs of students to the utmost, making his sentences clear and concise. His illustrations are nearly all of them wisely chosen, and while we think that some of the pictures of the lower forms of life, below the mammalia, might have been left out with advantage, this is not a point wherein lies a fault.

Dr. Mills has written a book which is, we fear, too good for its day. Unfortunately, necessity and desire for gain actuate so many ordinary mortals whose souls have no love for science in itself, that unless a book brings to them some practical suggestions for everyday life, they do not care for it. We doubt very much if the ordinary practitioner can gain much from its pages, and the average medical student has so many other text-books claiming his attention that we fear he will not thoroughly appreciate the one before us. The entire work is unusually well printed and bound, but is blemished by the publishers, who have inserted an illustrated catalogue of their publications which adds, with its index, sixty-three pages to the volume.

WOOD'S MEDICAL AND SURGICAL MONOGRAPHS. GENERAL ORTHOPEDICS, INCLUDING SURGICAL OPERATIONS. By AUGUST SCHREIBER, M.D., Surgeon-in-chief to the Surgical Division of the Augsburg Hospital. 8vo., pp. 345. New York: William Wood & Co., 1889.

THIS book, which forms part of the series of Wood's *Medical and Surgical Monographs*, is a valuable addition to the literature of orthopedic surgery. The frequency of deformities, and the great advances which have very recently been made in this branch of surgery, render a monograph on the subject of orthopedics and the modern orthopedic operations of value, not only to the surgeon, but to the physician, for it is the latter who in most cases has charge of the early treatment of these diseases. The first chapter is devoted to a consideration of general orthopedic treatment, mechanical appliances, and the surgical operations for the cure of deformities. In the subsequent chapters the special kinds of deformities and the conditions which produce them are considered in detail.

In the treatment of abscesses resulting from caries of the vertebræ, the author recommends the method of aspiration followed by iodoform injections. The statistics of P. Bruns, who had twenty permanent recoveries among twenty-two cases submitted to this treatment, should certainly encourage us to follow his method.

In the chapter on scoliosis due importance is given to the static form of lateral curvature, or to the curvature caused by unequal length or supporting power of the lower extremities. Dr. T. G. Morton has shown the frequency of this cause of lateral curvature in America, and Stoppel has found the same cause present in 76 out of 230 cases. The illustrations of scoliosis are good, and the discussion of the mechanical forces which produce

not only the lateral curvature, but the spiral twisting is thorough and interesting. As the author states, the conception of scoliosis, as a gradual transformation of the previously well-formed spine under mechanical influences, has at present the greatest number of adherents. We take pleasure in reading the advice as to the importance of early diagnosis and treatment in beginning scoliosis, and we heartily agree with the author when he states that "the indifference with which the anxious mother is quieted by the physician consulted for the 'high shoulder' or the 'oblique attitude' of a child with the statement that this will be 'outgrown,' and spontaneously disappear without even making a careful examination, cannot be too strongly condemned."

Chapter V. is devoted to a thorough consideration of deformities of the extremities. The operations of osteotomy for the relief of genu-varum and genu-valgum are clearly described; and the advice in regard to cases suitable for this treatment rather than for gradual reduction by mechanical appliance, is careful and conservative. The statement that the mortality after Macewen's operation for genu-valgum has been three out of 1384 operations, shows the safety of osteotomy under antiseptic methods.

The bibliography which appears at the end of each chapter is a very interesting feature. Our only criticism is that the references are chiefly to German literature: many American and English authorities being neglected.

WOOD'S MEDICAL AND SURGICAL MONOGRAPHS. Vol. III., No. 3, August, 1889. THE TREATMENT OF SYPHILIS AT THE PRESENT TIME, by VON ZEISSL. THE TREATMENT OF INEBRIETY, by STEWART. MANUAL OF HYPODERMIC MEDICATION, by BOURNEVILLE AND BRICON. 8vo., pp. 286.

THIS is one of the best numbers of this series that have so far appeared. In the first place, it is filled with just what the practical physician wants, namely, studies in the treatment of disease. The article by von Zeissl is, we think, unusually clear and to the point. The various modes of treatment are discussed in a fair and impartial manner; the results reached by each school of syphilographers are detailed, and the value of each method clearly pointed out. We do not think that we have ever read in so short a space such valuable material on this subject.

The article by Stewart on Inebriety in the Higher Classes occupies only eleven pages, and is somewhat disappointing. It attempts to deal with a difficult subject in an easy way, and fails. Most of the measures are only what common sense will dictate, and he suggests no new means of treatment.

By far the most important part of the volume is that on Hypodermic Medication, occupying as it does the greatest number of pages, and giving the reader one of the best *résumés* of this subject that has been published. We think that the publishers have been peculiarly fortunate in having given to the English-speaking practitioner such an interesting and useful work.

Many of the articles for use, mentioned, are extraordinary and never so employed, but as the text deals with everything that has, can and cannot be injected under the skin, it is proper that this should be so. The history of the subject is given thoroughly, without being too long and wearisome, and adds materially to the interest of the article. The translator, Dr. Currie, has added useful comments upon those points where he thought they were needed. The article is one which every active practitioner should read and profit by.

NEWS ITEMS.

Medico-Chirurgical Faculty of Maryland. — The semi-annual meeting of the Medical and Chirurgical Faculty of Maryland was held at Hagerstown on November 12th and 13th. The address of welcome was delivered by Dr. A. S Mason, of Hagerstown, followed by remarks by the President, Dr. A. Friedenwald, of Baltimore. Papers were read by Drs. J. J. Chisolm, Robert W. Johnson, Edwin Michael, George H. Rohé, William B. Canfield, Joseph T. Smith, Randolph Winslow, T. A. Ashby, S. T. Earle, William Lee, J. N. Mackenzie, and George J. Preston.

Chinese Army and Navy Service.—Cable dispatches state that William N. Pethick, Vice-Consul in charge of the United States Consulate at Tien Tsin, China, has notified the Department of State of an important step taken by Li Hung Chang, Grand Secretary of State for China and Viceroy of the Province in which Tien Tsin is located. Li Hung Chang has decided to establish a medical service for the Chinese Army and Navy on Western models. That he may have the best of these to study, he has asked Mr. Pethick to solicit the help of the State Department in obtaining a complete collection of the present regulations and various official publications for several years past of the medical department of our army and navy, including the organization and management of hospitals and ambulance service, and the Surgeon-General's report on our late war. The proposed service is to be under a foreign Surgeon-General with an adequate staff of assistants. It will include hospitals and dispensaries at various places, a medical school, and native surgeons for the fleet and the military stations of North China. A start will be made, the Vice-Consul says, with a number of young men, educated in the United States several years ago by the Chinese Educational Mission, and who have studied medicine under foreign teachers since their return to China. In accordance with the suggestion of the Vice-Consul, the State Department will ask the War and Navy Departments to provide the documents requested to be transmitted to Li Hung Chang.

The New German Pharmacopœia.—In October the Reichs-Pharmacopöie Commission (Imperial Pharmacopœia Commission) met in Berlin to frame a third edition of the *Pharmacopœia Germanica*, so as to have the work completed for 1890. The scientific discoveries or inventions, and the practical experiences made known in materia medica since the publication of the second edition, will in this forthcoming one be incorporated and rendered available. At the same time it will not undertake, any more than its predecessors, to set forth all those remedial agents which individual German physicians have prescribed and apothecaries have made up. In the preparation of these remedies the most diverse rules and prescriptions have been followed, resulting in inconveniences and errors, sometimes serious to the public

and compromising to the profession. To obviate or minimize this confusion, the German Apotheker-Verein (Society of Apothecaries) has resolved to issue a supplement to the forthcoming Pharmacopœia, which shall deal with all the remedies not given in the official dispensatories, and put the prescriber *au courant* with the latest and most accredited additions to his resources, both as to chemical quality and preparation. The profits accruing to the Society from the sale of this supplement will, it announces, be devoted to a philanthropic purpose.

University of Michigan.—The Ann Arbor *Courier* of October 30, 1889, states that "the special correspondent of THE MEDICAL NEWS credits Dr. Maclean with being in this University nineteen years, when it should be fifteen years. He also credits Dr. Maclean with building up the medical department of the University, when the fact is that the medical department of the University built him up. He was 'discharged' for repeatedly opposing the plans of the Regents of the University, and not because he opposed the retaining of the clinic in the little town of Ann Arbor; and the statement that Dr. Frothingham was elected President of the State Medical Society because of his views favoring the removal of the clinic to Detroit is not true, either. The Doctor was elected to that position because he was entitled to it, having withdrawn the year before in favor of an older man, and also because of his eminence."

Death from Ether.—A death under ether is reported to have occurred at Bellevue Hospital on November 5th. The operation was for abscess in the cervical region. Ten minutes after anæsthetization was begun, asphyxia took place. The kidneys were found, at the autopsy, to be the seat of cystic degeneration.

Resolutions on Death of Dr. C. L. Weed.—At a special meeting of the Deutsche medizinische Gesellschaft of Philadelphia, held on October 17, 1889, at the house of the secretary, the undersigned committee was appointed to convey to the family of our late fellow-member, in the name of the Society, an expression of heartfelt sympathy in their present affliction. The sense of personal loss sustained by the members of the Society, encourages the committee to venture the sending of these words of regret.

Dr. Charles L. Weed was one of the founders of this Society; and its present success is largely due to his untiring energy and to the signal ability with which he contributed to its scientific advance. His attainments were unusual. His premature death broke the promise of a rapid elevation to the highest ranks of our profession. His talents claimed our admiration; his geniality won our friendship. We will cherish his memory, and deplore his loss.

GEORGE A. MUEHLECK,
JOHN S. MILLER,
THOMAS H. FENTON, *Secretary.*

At a special meeting of the Staff of the North Fourth Street Union Dispensary, held October 17, 1889, the undersigned committee was appointed to convey an expression of sincere sympathy to the family of their late co-laborer; and a resolution, indicative of the great loss experienced by them on his death, was adopted.

Dr. Charles L. Weed was chief of the eye and ear department, and his ability and energy built up a most successful clinic in that field. His loss will be keenly felt, both by his patients and colleagues, among whom he was deservedly held in high esteem.

Knowing his character and ability, and high qualities both mentally and morally, we realize most forcibly the great loss his death has been to all, and we can and do offer to his family, in the name of the staff, our most heartfelt sympathy in their affliction.

J. REED CONRAD,
JOHN S. MILLER.

Successful Cæsarean Section.—According to the *Lancet*, DR. MURDOCH CAMERON, obstetrician to the Glasgow Maternity Hospital, has recently performed his third successful Cæsarean section. The patient was rachitic, about 48 inches in height, and had been in labor two days when the operation was performed. Both mother and child were saved.

Recent Appointment.—DR. FRANCIS J. QUINLAN, of New York, was recently appointed Lecturer on Diseases of the Throat and Nose in the New York Polyclinic School and Hospital. He is connected with the Eye and Ear Hospital, and is Assistant Surgeon in the Department of Diseases of the Throat and Nose in the Vanderbilt Clinic of the College of Physicians and Surgeons.

New York Post-graduate Medical School and Hospital.—The Executive Committee of this institution have established a clinic for diseases of the rectum, to be under the care of Dr. Charles B. Kelsey, for the treatment of poor persons suffering from these diseases. Dr. Kelsey will also give clinical instruction in the Post-graduate School on this subject.

It is believed that this is the first institution in New York City to organize such a clinic, which has long been needed. The wide reputation of Dr. Kelsey, founded upon years of special work, will afford a guarantee that the cases will be skilfully treated. Dr. J. Blair Gibbs will assist Dr. Kelsey in this new departure.

The New York Academy of Medicine.—The Anniversary Meeting of the New York Academy of Medicine was held on Thursday, November 21, 1889. The address, entitled, "The Relation of Medicine to some of the Questions of the Day," was delivered by William M. Polk, M.D. This meeting was public, the laity—ladies and gentlemen—being present by invitation.

Yellow Fever at Key West.—DR. JOSEPH Y. PORTER, State Health Officer of Florida, states that about September 21st a case of yellow fever was recognized by the surgeon of the Marine-Hospital Service stationed at this port, in the person of Abraham Horn, a peddler, who was at the time convalescent.

On the 5th of October the same medical gentleman recognized two cases of yellow fever in the persons of Wolff Abraham and his wife; the wife being convalescent, the husband being still in bed.

These above-named cases were reported to Surgeon-General Hamilton as yellow fever. The fourth case was recognized by Assistant Surgeon Geddings, of the Marine-Hospital Service, and Dr. Porter, on the twelfth day of illness, the attending physician, Surgeon Murray, dissenting from this diagnosis.

The fourth and last case is now sufficiently recovered to leave his house, and as it has been three weeks since his seizure and no other cases have arisen in his family, all restrictions of egress and ingress have been removed from the premises.

Every precaution has been used to prevent the spread of the disease, and the infected bedding, and other articles liable to carry infection, that could not be subjected to moist heat have been destroyed and paid for. The premises have also been disinfected and cleaned. The city has been ordered to be cleaned, and all the foul spots disinfected, under the direction of the city health officer, who to-day reports the completion of his task.

At the time and immediately preceding the occurrence of the fever cases there had been an unusually heavy rain-fall at this place, followed by a period of intense heat of about a week. Happily this was of brief duration, and being followed by cool north winds had the effect, to his mind, of arresting any spread of the fever. For the past two weeks the weather has been unusually cool for this place at this season.—*Weekly Abstract of Sanitary Reports.*

Sickness Relief Medical Association.—A mutual benefit organization exists in England for the relief of medical men who fall sick or suffer accident. It is known as the Medical Sickness and Life Assurance Society. It addresses itself chiefly to the junior members of the profession and to those practising in the country districts. It has a capital of $140,000, and is directed chiefly by medical men who serve without pay.

The California State Board of Health.—The legal contest of Dr. J. R. Laine to recover his seat in the California State Board of health, now held by Dr. G. G. Tyrrell, has, according to the *Occidental Medical Times* for November, resulted in the favor of Dr. Laine. However, the defendant will move for a new trial, and delays without end may ensue before the latter can be compelled to surrender his position.

Faith-curing of Contagious Diseases Suppressed.—The Board of Health of Matteawan, New York, having encountered a case of diphtheria that was being neglected by some faith-curing practitioners, declared an immediate quarantine, which was maintained by the police. In this manner the faith-curists were shut out, a reputable physician put in charge of the case, and the child began to improve. Apparently its life has been saved by the expedious action of the sanitary board.

The Paris Exhibition.—American pharmacy has been worthily represented at the Paris Exhibition by Messrs. William R. Warner & Co., of Philadelphia, who were accorded the silver medal of their department (the highest award) for their pharmaceutical specialties.

Danger in the Use of the Telephone.—The frequency with which telephone lines become crossed by electric-light wires carrying strong currents seems to be constantly increasing. Dr. Albert R. Rice, of Springfield, Mass., recently found the wood-work surrounding the telephone in his office on fire, owing to this cause. It was discovered just in time to prevent the destruction of his house. The application of such an instrument to the ear would, of course, produce fatal results, and while many telephones in the large cities have attachments which are supposed to prevent such accidents, the danger is certainly one which should not be overlooked.

Isolation of Dangerous Industries.—The French government is investigating the manufacture of celluloid, which has been found too dangerous to life and property to be unrestricted. Within three years there have been three or more accidents, with loss of life, serious injuries, and destruction of property. The large quantities of gun-cotton at the celluloid works makes the business extra-hazardous, and unfit to be permitted in settled communities. The terrible explosion at a factory in New Jersey, about two years ago, was an unmistakable lesson on the importance of placing such establishments in isolated localities.

The Census of 1890.—Commissioner Porter, of the Census Bureau, has made the following statement regarding the new features that may be looked for in the coming census:

"Owing to improvements in the records of some of the States and to the active coöperation of 80,000 physicians, to whom registers have been sent, the vital statistics will be more complete than it was possible to make them in the census of 1880.

"Among the important new features is a special study of the birth- and death-rates and of the principal causes of death in twenty-four of our largest cities; a special study of the influences of race upon fecundity and mortality, and a special study of the relations of occupation to death-rates and to particular causes of death."

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM NOVEMBER 12 TO NOVEMBER 18, 1889.

The following changes in the stations of medical officers serving in this Department are hereby made:

L. W. CRAMPTON, *Captain and Assistant Surgeon.*—Ordered from Fort Teyon, Colorado, to Fort Sheridan, Illinois.

W. H. CORBUSIER, *Captain and Assistant Surgeon.*—Ordered from Fort Hays, Kansas, to Fort Lewis, Colorado.

IVES, F. J., *First Lieutenant and Assistant Surgeon.*—Ordered from Fort Teyon, Colorado, to Fort Sill, Indian Territory.—Par. 3, *S. O. 167, Headquarters Department of the Missouri, Fort Leavenworth, Kansas,* November 9, 1889.

By direction of the Secretary of War, LOUIS M. MAUS, *Captain and Assistant Surgeon,* having relinquished the leave of absence on surgeon's certificate of disability, granted him in S. O. 249, October 25, 1889, from this office, will, upon being relieved from duty at Fort Porter, New York, as directed in S. O. 242, October 17, 1889, from this office, proceed without delay to Fort Stanton, New Mexico, and report in person to the commanding officer Department of Arizona.—Par. 8, *S. O. 261, Headquarters of the Army, A. G. O, Washington,* November 8, 1889.

THE MEDICAL NEWS *will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.*

Local papers containing reports or news items should be marked.

Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

All communications relating to the editorial department of the NEWS *should be addressed to No. 1004 Walnut Street, Philadelphia.*